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THE PSYCHOLOGICAL REVIEW

ERRORS IN THE CRITIQUES OF GESTALT PSYCHOLOGY III. INCONSISTENCIES IN THORNDIKE'S SYSTEM[1]

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This paper, following upon Number II, discusses problems raised by Professor E. L. Thorndike. Professor Thorndike's psychology of former years is too well known to review here. Suffice it to summarize the major assumptions upon which it was based. First, it was strictly mechanistic. It assumed that any total or whole was merely a sum of its parts and that complex activities were built up from parts or elements which, by implication, were unrelated to each other in the beginning. This is proved, of course, by the fact that orderliness, that is, relations, were of necessity brought into existence by mechanistic means. They did not exist at the outset. The purpose of the laws of use, disuse, effect and readiness was to explain the origin of these relations. Second, man came into the world endowed with native responses, each one discrete, an instinct, or a reflex. Thus, inherited were sharply distinguished from acquired, characteristics. Third, when the organism confronted a new situation with respect to which its inherited repertoire of activities was inadequate, behavior

[1] Number I of this series appeared this Journal, 1931, 38, 109–136; number II appeared 1933, 40, 221–245.

was chaotic and random. Fourth, total situations were recognized as facts, but in responding to a total situation the organism reacted directly, not to the situation as a whole, but to elements in the situation. They, the elements, contributed their own, independent influences. There is no suggestion that the property (for the organism) of one situational detail depended upon the properties of other details in a fashion postulated by the concept of configurational response. Fifth, the organism may respond as a whole but it responds in parts first and, as a whole, second. This is always the implication, if not the direct assertion, when organisms are supposed to react by means of instincts, reflexes, or in terms of other elemental and mechanistic units. Why, in the first place, these units should behave as they do, is a vital question which Thorndike, or for that matter any mechanist (atomist) finds impossible to answer.

In recent years, however, Thorndike has added much to his system. It is the purpose of this discussion to show that without exception each advance substantiates *Gestalt* psychology. Consider, first, his discussion of *Human Learning*.[1a] There is an effort at the outset to evade the whole problem of organization as it applies directly either to mind or to the nervous system. This is accomplished by setting up a purely statistical definition of the S–R bond. "The word connection has been used without prejudice concerning what physiological event or condition parallels or constitutes it."[2] But this is not true. We are soon given a physiological theory of the bond. "The neurons are . . . by the hypothesis, widening the gaps in those synapses conduction across which causes discomfort; and are maintaining those spatial relations . . . conduction across which causes satisfaction."[3] "The law of effect would be a secondary result of the *ordinary avoiding reaction* of unicellular organisms co-operating as elements in the animal's brain."[4] This is of course no more than a restatement of the old law of readiness, in its extreme,

[1a] The Century Company, New York, 1931, thereafter referred to as T 1.

[2] T 1, 7.

[3] T 1, 58.

[4] T 1, 58.

atomistic form. The purely statistical definition of the bond is thus forsaken and an effort is made to conceive a natural bond in terms of an atomistic agent. "Then the modifiability or connection-changing of a neuron equals its power to alter the intimacy of its synapses" [5] and the learning of an animal is an instinct of its neurons. Admittedly, such a hypothesis is speculative "but it is not mysterious. Such a dynamic of learning could exist and operate, though I can offer no important evidence that it does." [6] And good reason. It is impossible to offer such evidence. Thorndike is hunting in the wrong places for causes.

The best way to test a theory is to accept its logic, and then to work with it. Assuming Thorndike's speculation to be true, serious difficulties arise at once. It is a well known physiological fact that both avoidance- and acceptance-reactions require preferential use of certain muscle groups, but the innervation of certain muscles and inhibition of others depends upon a unified type of central control. The widening of certain synapses and the closing of others through amoeboid movements could not possibly be the criteria, respectively, for annoying and satisfying reactions. Both must occur in producing a single avoidance reaction or a single acceptance reaction because certain contractions must take place at the expense of others within the same movement.

Moreover, why should a widening gap produce or parallel the feeling of annoyance or a closing gap a feeling of satisfaction? Annoyance occurs in a painful situation or when a goal activity is thwarted. These are situations in which the organism must be highly active. On Thorndike's own hypothesis there would be required at this time *a maximum of muscular innervation and a maximum number of closed synapses.* But since annoyance is conditioned by widened synapses it would defeat its own purpose. Further, the pain or annoyance depends upon receptors, and any theory presupposing pleasure or displeasure to be conditioned solely by central processes is totally unintelligible.

[5] T 1, 59.
[6] T 1, 60.

Thorndike's inconsistencies are not confined to his own theories. He attacks *Gestalt* conceptions only by setting up as the object of his criticism something that is not *Gestalt* psychology at all. For example, after illustrating certain acts of learning he remarks, "these are samples of thousands of sorts of learning where the unity is the simple belonging of situation and response, not an unanalyzable Gestalt." [7] Thorndike has distorted the configurationists' definition both of *Gestalt* and of unanalyzability. A *Gestalt* is not an element; it is either a complex experience or a pattern of movements whose unanalyzability lies only in the fact that it is destroyed by analysis. It is not unanalyzable in the sense that it cannot be broken down.

It is curious that Thorndike should have implied a criticism of *Gestalt* in the same sentence in which he accepts a principle adapted from *Gestalt* psychology, namely, that of belongingness. At the same time, his use of the concept which, by definition, presupposes the primacy and inviolateness of a whole, denies the principle of wholeness. 'Belonging' means '*already* in dynamic relation to.' This presupposes organization and unity, an all-or-none situation. One thing does not belong to another if it can exist without it. And yet Thorndike attempts to build up belongingness by connections that presuppose starting without unity. His belongingness in so far as it is self consistent is an attempt to express exactly what the *Gestalt* psychologists have been expressing all along in their concept of 'membership character.'

Thorndike's distortion of *Gestalt* psychology comes to light, again, in the statement, "there are very long series of sequences related not as the parts of a configuration to its total, but as incidents in a general plan, or as steps in a useful routine." [8] Immediately after the word 'but,' he admits exactly that which he denies in the first part of the sentence. What are incidents in a general plan, steps in a routine? The plan comes first or the incident would not

[7] T 1, 130.
[8] T 1, 127.

occur in the plan; the routine comes first or there are no steps, just as a half an apple cannot exist previous to a whole one. 'Step' is defined as part of a whole by calling it a step. Would two boards be a step if they were not, when discovered, part of a stairway? It is the stairway that defines the boards as steps. A certain routine is a unitary plan organized toward an end, and the step has meaning, in fact existence, only in terms of the plan. *Gestalt* psychology explains both why and how the whole comes first and the parts come second; the step differentiates from the plan which, no matter how undifferentiated, is a unified whole from the outset. The whole expands and differentiates.

Thorndike's troubles with *Gestalt* principles appear in his criticism of Ogden. For example, 'the behavior sequence of *playing with blocks—awareness of mother with cooky*' is something which, according to Thorndike, Ogden could not explain.[9] Thorndike implies that such a behavior on the part of a child is something genuinely different from a configurational response, because the behavior leads to something new rather than back to a previous state. But it is one of the primary principles of *Gestalt* dynamics that a behavior sequence leads to something new and at the same time to equilibrium. Thorndike distorts *Gestalt* theory and then declares that it will not work. Then, in order to make his own conception of belongingness work he must forsake the meaning which he himself gives to it. According to Thorndike, playing with blocks and then becoming aware of mother involves *a break in belongingness* when, by definition, belongingness permits of no break. Ogden's own explanation had already solved the problem which Thorndike's conception of belongingness failed to solve. 'Playing with blocks' and 'mother with cooky' *are parts of a larger whole* which Thorndike has overlooked, namely, a home situation, or a room situation.

In this same connection Thorndike misconstrues the configurational definition of an end or goal as something absolute. The child, in his illustration, behaving under tension, was re-

[9] T 1, 127.

solving that tension by playing with blocks; then mother appeared. The total situation changed with the appearance of mother. This changed the tension. The potential was shifted by a change in the stimulus-pattern, which means that the end was also changed. A potential changes only when an end changes and *vice versa*. Both the end and the potential come into existence and go out of existence together.

There are other troubles with *Gestalt* psychology. "I cannot tell wherein the configurative brain-process which makes a person find the opposite of a given word differs from that which makes him find the product of two given numbers. . . ." [10] Thorndike is looking at a configuration back-end to, the typical mechanistic error. He seems to think that in order to explain the differences, it must be shown how the parts of the two separate responses *get put together differently*. There is no theory that will tell him how parts get put together. *Gestalt* psychology shows how the configurations are different by being differentiated differently from, and by diverging differently within, an original, unified field. The differentiation and divergence occur in terms of the different types of stimulation which the organism receives. Both Thorndike and the *Gestalt* psychologist appeal to a stimulus-situation, *but for opposite reasons*. In Thorndike's case the stimuli arouse experiences that must be combined, or fitted into the whole from outside; the *Gestalt* psychologist appeals to stimuli as factors which induce the differentiation of specific experiences out of an undifferentiated, but unitary groundwork of mental life. Behavior- and experience-patterns are differentially structured under laws of dynamics; they are not put together under laws of association.

In his general attack upon the *Gestalt* position Thorndike appeals apparently to neurology, when its recent advances favor the configurational standpoint.[11] The new neurology shows how behavior is *not* to be explained by the forming of connections. Responses individuate from mass-action, *in-*

[10] T 1, 125.

[11] T 1, 126, ff.; Coghill, G. E., Anatomy and the problem of behaviour, New York: Macmillan, 1929.

tegrated as they emerge. They are integrated because they are functions of unified dynamic fields. Thorndike assumes physical unity as an end-product. Neurology now assumes it as a constant, running through all the genetic stages of the individual's growth. It accepts unity as a principle to be employed in explaining learning, not something which learning explains.

Thorndike's confusion as regards the true relation of connectionism to configurational thinking is portrayed, once more, in the following quotation. "The configurationist is in a stronger position when he accepts the formation of connections as the explanation of much learning, reserving configuration to be a principle of organization." [12] The quotation implies that organization is an end-product, derived from a situation which originally lacked organization. In his concept of belongingness, Thorndike assumes that organization is primary, or axiomatic, while in his principle of 'connection' he assumes it to be secondary or derived. He is therefore in the dilemma of treating unity at times as something to be explained and at times as something requiring no explanation, when it is the same unity all along.

There is another difficulty with connectionism. It does not explain the most important aspects of movement, namely, its beginning, its direction, and its end. Indeed, by definition, it defeats its own purpose. The general law of association expressly admits this defect. "After B has followed A once, it is more likely to follow A the second time." By implication it says "if (or when) B follows A once, B is more likely to follow A a second time." It is merely an accident that B followed A the first time. In admitting that B did follow A the first time the connectionist grants direction to the activity but is in a helpless situation when asked to explain from what source the activity obtained its direction. Merely to say that the nerve current ran along the nerve fiber does not explain its direction, because it does not explain why the nerve fiber happens to be a conductor. To say that an electric current travels over a wire does not explain the

[12] T 1, 130.

direction of the current. There must be, at the outset, a unified, polarized field, structured into high and low charges, or potentials, before either a wire or a nerve fiber becomes a conductor. The direction of nerve conduction is derived from the polarization of a neural field into stresses that correspond to high and low. The existence of stresses precludes an aggregate of isolated parts between which connections must be formed. The 'connections,' that is, relationships between the poles, are already there, 'formed' at the time the stresses are differentiated by the influence of outside forces. The same stimulus-situation which sets up a high stress also sets up a low, just as it is impossible to heat one flatiron, lying against another, without making the other cold with respect to it. Thus, a disturbance outside a given field sets up a differential and the differential gives direction to movement within the field, under the law of least action. We are thrown back upon configurational explanations, therefore, to account for that which connectionism cannot explain, and, in terms of its own assumptions, denies.

Turning now to the *Fundamentals of Learning*, there is much that should be said by way of commendation. The volume contains a mass of extraordinarily valuable experimental data. It is necessary, however, to assume that what is good in this volume will speak for itself. We are at the moment interested in the discrepancies between Thorndike's own results and his interpretations as seen from the standpoint of *Gestalt* psychology. We begin with Chapter Two on the influence of repetition. In discussing the possible physiology of nerve conduction, Thorndike remarks, "the repeated conduction of a stimulus over the same path actively enriches the tendency so to conduct at the expense of tendencies to conduct elsewhere. Or it may be some sort of selective integration whereby the repeated action of more or less of the associative system as a whole in a certain pattern depresses the tendency for it to act in other patterns." [13] Nowhere through the volume can we find a fair test, on Thorndike's

[13] E. L. Thorndike, The fundamentals of learning, New York Bureau of Publications, Teachers College, Columbia University, 1932, p. 7. Hereafter referred to as T 2.

part, of the second alternative. This, obviously, is because he is predisposed by mechanistic assumptions to find, if possible, a proof of the building-up-by-piecemeal theory. Furthermore, why does Thorndike call the nervous system as a whole an associative, rather than a fluid, dynamic system, in which association does not figure? A system, functioning as a whole, does not involve the associating together of pre-existing parts. There are no parts, existing separately, prior to organization, therefore, no bonds to be formed, nothing to become associated together. The parts are already members of a unified field each part doing its work in accordance with demands upon the whole, and under laws of balance.

All through the volume Thorndike assumes connections, and connection forming. His interpretations are then dictated by his assumptions; his results do not prove the assumptions. For example, in experiment 1 where the subject attempted to reproduce lines of given lengths, the results show a tendency toward stereotypism. This in itself is no proof of connections. There are laws of dynamics, explaining stereotypism, laws which preclude connection forming as an assumption. They are the laws of least action and maximum work. The neuromuscular system as a whole, under continuous stimulation, functions in such a way as to reduce its energy expenditure to a minimum, and at the same time to preserve its unity and organization. Simple physical systems like atmospheric, electrical, and gravitational fields, obey these same laws, but where are the connections? They do not exist in energy systems and the brain is an energy system. But Thorndike remarks, "the connections (assuming them all the while) involved in estimating these lengths do not seem to remain in *status quo*." [14]

[14] T 2, 11.

In passing, a comment should be made on a footnote, page 18, in which Thorndike attempts, apparently, to show that he is not unmindful of wholes. It is of no avail to recognize wholes unless one also recognizes the principles in accordance with which wholes function as primary units and not as complexes composed of elements. In his mere recognition that total situations exist, Thorndike is a long way from preserving his system against its mechanistic assumptions. For example, can the mechanist explain how previously existing unrelated and independent parts get put together in a building-up process, when, by definition, there is no means, no relationship between

The results of experiment 23 are interesting. Where "the situation was a word heard, the response was the writing of a digit from 0 to 9." [15] It was found that certain numbers came to be used exclusively in response to certain words. This is traced by Thorndike to the satisfyingness of a ready response fixed in memory. What better proof does one need of the effort, by the learner, *to make his task as easy as possible.* Indirectly, Thorndike is demonstrating the laws of determined action and least action, but the whole that governs the activities of its parts has escaped him.

In experiment 26, Thorndike approaches the *Gestalt* position when he remarks, "the gains are in general in the direction of more reasonable responses." [16] He admits the true explanation but, instead of using it, he goes on to say that "the changes are better explained by the strengthening of the more satisfying connections." [17] What, after all, is the value of satisfyingness unless the organism knows, first, that the response is successful or that it is easier? In other words, which comes first, insight or satisfyingness?

In experiment 27, Thorndike discovered "that the few-letter responses grow at the expense of the many-letter responses." [18] This is another excellent illustration of least action. The few-letter responses are easier merely in terms of raw energy expended, regardless of satisfyingness. Thorndike accepts a more complicated explanation in favor of a simpler one, a typical mechanistic abuse of the law of parsimony. Seeking evidence for the law of effect, he finds that the few-letter responses grow because of their greater ease. Curiously, Thorndike mentions ease, a problem in dynamics, but subordinates it to satisfaction *although ease is mentioned first* in the quotation, thus, 'ease of writing a short completion and the satisfaction of finishing a sheet quickly in the presence

the parts in terms of which the getting together can be effected? A medium through which this can be achieved is ruled out by assumption. There is no system of thought so saturated with mysticism as the mechanistic one and yet the mechanist is the first person to condemn the organismic position as mysticism.

[15] T 2, 27.
[16] T 2, 48.
[17] T 2, 48.
[18] T 2, 51.

of others.' [19] Is the performance pleasant because it is easy, or is it easy because it is pleasant?

Interesting results were also obtained from experiment 29, on belongingness. The more complete the stimulus-pattern included in the final test, the more accurate the answer, again confirming a principle in field-dynamics, namely, that the more complete the stimulus-pattern, the more uniform, accurate and complete the reaction.[20] But again Thorndike takes the more complicated, and the self-contradictory course, and assumes connections. He is forced to admit that somehow certain of the connections were not straightened out and others were. But why? He has no answer, except in terms of belongingness which is properly a configurational concept.

Thorndike's troubles with belongingness mount when he faces the question of its physiological basis. He admits, from his standpoint, that there are at present no hypotheses about it, failing to recognize that *belongingness is what organismic physiology is all about.* Yet he explains, "The one (hypothesis) which I offer is the very simple one that belongingness is the consequence of direct continuity in conduction." [21] Thorndike has the cart before the horse. The direction of nerve conduction is traceable to the polarization of a unified, fluid field and to lines of least action; hence *the direction of conduction and its continuity are the consequences of 'belongingness'; 'belongingness' is not the consequence of them.*

Experiment 40B, on impressiveness, showed results which check very well with configurational assumptions. Impressiveness is an indication of the amount of tension set up by the stimulus, and of the relationship of the word used to the individual's behavior-pattern as a whole. The stimulus has emotional value, and, according to organismic principles, emotions represent high levels of tension, the rapid achieve- of something in the immediate situation in terms of increased

[19] T 2, 52.

[20] *Cf.* The theory of memory, suggested by Wheeler, The science of psychology, New York, Crowell, 1929, p. 270 ff., and Wheeler and Perkins, Principles of mental development, New York, Crowell, 1932, p. 388 ff.

[21] T 2, 76.

energy. The law of least action explains the increased achievement under increased energy, and yet Thorndike remarks "these facts furnish strong support for an associational or connectional mental dynamics as opposed to such a dynamics as the *Gestalt* psychologists seem to advocate, since the first half of the belonging sequence behaves so differently from the second half." [22] That is, when pairs such as *kiss-63* and *vomit-21* are given, a repetition of the word was more fruitful in leading to correct responses than a repetition of the numbers. Of course. *This is precisely what is to be expected under Gestalt dynamics.* A response is a unit in time as well as in space. If it is broken up, as when repeating the number without the word, the larger whole, the word paired with the number, is not induced by the stimulus only; a smaller whole is set up, the number. The only way to differentiate and thus to 'fix' a configurational response is to control the stimulus-pattern in such a way that the unity of the response is not affected. In fact, configurationally, one would predict that, repeating the first phase of a continuous response would, within limits, help the total response to differentiate. Thus, in learning the pair, *wall-16*, restimulating with 'wall' will make it easier to remember *wall-16*. This is because a response demands completion and 'wall-16' is the complete response. Furthermore, "certainly nobody would ever have prophesied that repeating the first parts would be as effective as repeating the second parts." [23] Just this point *had* already been mentioned, especially in connection with the learning process in children. If confused when in the middle of a response, children insist upon returning to the beginning and repeating it. A seven-year-old child is spelling 'robin.' He arrives at the letter *b* and cannot remember the next letter. But when he returns to the beginning the sequence 'rob' frequently completes itself without effort and the word is spelled correctly. The same observation can easily be made on the reading process in children. Adults learn in the same fashion if they really learn at all.[24]

[22] T 2, 144.

[23] T 2, 147.

[24] *Cf.* Wheeler and Perkins in a discussion of reading, Chap. 24, Principles of mental development, New York: Crowell, 1932. Also Koffka's Law of recall, Growth of the mind, New York: Harcourt Brace, 1924.

A brief recognition of organismic principles is given in Chapter VII on the polarity of so-called mental connections. There is no other way, of course, of handling the problem of polarity except in an organismic fashion. The term polarity implies unity and all that goes with it. It is an interesting commentary on Thorndike's understanding of *Gestalt* principles that, on page 158, *he proceeds to contrast polarity with a Gestalt conception when by definition polarity presupposes a unified, fluid field.* But contrary to Thorndike's use of the concept, unity, in this chapter, all responses, good and bad, possess the same degree of unity. One unified whole may have energy available for use when another whole, equally unified, does not. The difference lies in the type and complexity of structurization. Thorndike goes on to say, however, "Peekskill has a certain unity or all or none quality which 5J lacks." [25] So long as 5J exists at all it has, in proportion to the amount of energy involved in the response, exactly as much unity as Peekskill.

Coming now to the law of effect, Thorndike is handicapped in his criticism, this time of Köhler, by an inadequate understanding of configurational dynamics. He is attempting to improve upon Köhler's doctrine of equilibrium. This "doctrine of equilibrium applied to our case would mean either that, in learning, those after-effects which bring or restore an animal to a state of inner equilibrium or peace do strengthen the connections which produce them, or that connections themselves in general tend to become strong in proportion as they produce such inner equilibrium." [26] Thorndike has the cart before the horse, once more. Parts are again assumed to be the causes of wholes. There can be no testing of a connection by starting with the principles of equilibrium because such principles do not assume connections. One cannot test an assumption which he does not set up. In the configurational view, when an animal or a human being is motivated by a stimulus-situation which is pleasant or unpleasant, his general energy level is raised and, in so doing, the goal is

[25] T 2, 153.
[26] T 2, 184.

brought nearer to him, dynamically.[27] With more energy, and the goal dynamically nearer, there is more rapid learning, more vigorous achievement, increased insight, all of which are nothing more, in a last analysis, than expressions of available energy. And energy does not have 'connections'; it has unity.

In another experiment, No. 45, line drawing was employed again. First, the drawing took place with no announcement of right or wrong, second, with the announcement of right or wrong, third, without an announcement and finally, with announcement. There was some improvement, which Thorndike attributes to satisfyingness. The greatest success "has no advantage in congruity or harmony with the subject's purpose . . . save what the after-effect gives. . . . It has no advantage in completeness, finality, or consummatory quality, except such as the after-effect itself may give." [28] Thorndike seems to be quite positive in the face of facts to the contrary, facts he himself admits. He has already admitted, as quoted before, that the announcement of wrong gives the subject an idea of what he is doing. He also admits, "there is the possibility that the beneficial action of the reward . . . consisted, in whole or in part . . . of some methods or ideas or guiding sensations, and the deliberate effort to make such movements as were in harmony with these." [29] Furthermore he admits that announcement of right may leave the organism 'more in peace' and in 'freedom from tension.'

These circumstances are not in themselves the causes, however, according to Thorndike; they are the means by which satisfying after-effects strengthen the connections. Four times in succession, within the scope of one page, every possible explanation is twisted into a subordination to effect. Everybody knows from common experience that, when an act is being executed in an indefinite situation, rewards or punishments are accepted by the learner for the sheer purpose of increasing his knowledge of what he is doing. When

[27] *Cf.* Wheeler and Perkins, Principles of mental development, New York: Crowell, 359 f., and 115–208.

[28] T 2, 193.

[29] T 2, 189 f.

the announcement of wrong is given, the learner tries to ascertain wherein he was wrong. If the announcement of right is given, the learner tries to ascertain why he was right. If the announcement had been given, 'wrong, one quarter inch too long,' improvement would have been much more rapid.

According to Thorndike, we not only have obvious connections but hidden connections. Here Thorndike inadvertently falls again into the hands of the configurationists. Results are explained as a consequence of "not . . . taking a quick impressionistic look at the card and guessing. . . . Consequently, they do not form connections." [30] What is a 'quick impressionistic look' if it is not the perception of the card as a whole, or, in other words, a phenomenon of undifferentiated insight? Lack of insight, therefore, almost in Thorndike's own words, explains what he calls lack of connection forming. Connection forming is Thorndike's way of saying that one detail of a situation is perceived in relation to another. Is Thorndike willing to regard these relations as mystically manufactured out of whole cloth, or is he willing to take the simpler course and grant a fluid field, already 'connected' in all parts, where 'connection forming' is a redundancy? Does he realize that 'connection forming' presupposes an original chaos of unrelated parts where there are no wholes and therefore no means of getting the parts together?

Now we come to the influence of mental systems, a rather curious anomaly in a connectionistic psychology. "Uniformity, simplicity, and independence are desirable" agrees Thorndike. Thus the basic characteristics of unified dynamic fields, or wholes, are accepted, but the dynamics are ruled out.

"Present believers in forms of organization . . . would not presumably go as far as this (meaning the use of association systems). Since we are engaged, not in a debate, but in the search for the truth, we shall not impute any decision to them, but shall simply examine the facts to learn whether organization by systems, due to habit is adequate or inadequate." [31] Thorndike, truly, is engaged in a search for the

[30] T 2, 245.
[31] T 2, 365.

truth but the truth that he is seeking follows, if at all, from his assumptions and those only. He confesses it indirectly in the quotation just cited, for he will examine the facts to learn whether organization is not *explained by habit.* He dodges the issue *whether habit can explain anything or not.* He assumes that the facts will tell him the truth, regardless of the consistency or inconsistency of the assumptions by means of which he must evaluate the facts. Accordingly, we should expect, since his assumptions validate habit in his own mind, that the experiments will prove the efficacy of habit and this they do, in his own estimation. Other alternatives are precluded, by assumption. "On the whole, I conclude (1) that if we start with a general tendency for contrariety to direct thought, we find many facts impossible of explanation thereby; (2) that the facts found are explainable by special habits and systems produced by habits." [32] We are not told what the facts are that cannot be explained configurationally. Instead, it is assumed that systems do not originate configurationally, although they exist. It is assumed that because they exist they must be explained by habit. Has it occurred to Thorndike that systems might persist because they were configurational or, in other words, that habit is an effect, not a cause? According to configurational principles, habit is merely the perseverative aspect of systems resisting disintegration under the law of maximum work.

Further on we read, "it is a useless and harmful habit to treat persons as unanalyzable wholes when one is studying the fundamentals of behavior and learning. For such study we need to abstract certain particular connections from the total flow of life of which they are parts." [33] *This is precisely what the configurationists do, only Thorndike has not discovered that they do it in a different way.* Instead of beginning with artificial abstractions that do not exist in nature, the configurationist commences *with nature*, and *observes* her particular processes as they differentiate from previous states.

The difference between the mechanist and the configura-

[32] T 2, 371.
[33] T 2, 393.

tionist is this: The latter attempts to build up an artificial picture; the former attempts to describe a real and natural picture, taking as its basis nature's own objective processes of analysis rather than those man has erroneously conceived. Some of this is admitted by Thorndike: "the organization . . . does issue orders which . . . connections . . . execute." [34] If connections, after all, are subordinate to organization instead of organization subordinate to connections, why does he not organize his facts about such principles, as the configurationists have long been doing?

Lastly, Thorndike attempts to defend his system from adverse evidence and arguments. First, we have the inevitable assumption that repetition and satisfaction cause learning. That learning 'depends' upon repetition of response only because they are one and the same thing has not occurred to him. A typical instance of this misunderstanding appears in the statement, "training as such has a genuine dynamic effect; it causes a tendency to execute the movement learned if the situation permits." [35] Thorndike and the configurationist are on opposite sides of the cause and effect relation. It is only by assumption, one way or the other, which is cause and which is effect, so that the anwser must be thrashed out on logical grounds. The configurationist insists that repetition of response cannot have a dynamic effect because the repetition, in the first place, was the consequence of disequilibration and disequilibration was a consequence of outside disturbances. Further, any change occurring in the organism is a result, not of doing, but of the circumstances which induce the doing. The response or doing and the change are two names for the same thing. The greater probability that the act will be repeated is not traceable to former acts which were themselves effects, but to the causes that produced those acts. When the acts themselves are regarded in sequence, later stages in learning depend upon earlier stages, not dynamically, but definitionally. Thus, if one is thinking of a falling body starting from a certain position and reaching the ground, he presupposes the total act. But he may state

[34] T 2, 399.
[35] T 2, 437.

the problem in such a way as to mention the second half of the flight first. Then, of course, the ball will not traverse the second half until after it has traversed the first half. There is a dependence of the second half upon the first, but it is not dynamical, it is definitional. This is true of any sequence. A later member depends upon an earlier member not in relation of cause to effect but in both being presumed at the same time. Moreover, the nervous system is an energy system and it is well known that energy does not follow laws of use and disuse. Whether an act will occur or not depends upon the existence of differentials; then the acts are effects not causes.

We go on: "The repetition of the connections between certain syllables and certain movements did have real effects." [36] Did they? Only in terms of Thorndike's own assumptions. There are reasons why responses persist other than the supposed one of the formation of connections. "Indeed, unless my reading has been careless, the repeated connections displayed potency in every test." [37] The repeated connections displayed potency only because, for Thorndike, connections must be repeated in order to explain learning. Proof of it, in Thorndike's estimation, comes from the following: "If the subject is distracted so that he neglects the present instructions, he usually makes the old movement." [38] This fact does not demonstrate connections at all. It merely, in Thorndike's opinion, fits his assumptions. From the configurational standpoint perseveration is an instance of a dynamic law quite similar to that of Newton's first law of motion. When something starts, it keeps on going until something stops it. It is also an instance of 'maximum work' which states that an energy system will resist change, not because there are connections to be formed or that have been formed, but because of unity in the whole (the whole follows laws of balance) and because the behavior of the parts is determined by the whole.

"Fact after fact in his [Van der Veldt's] thorough investigation shows the potency of repetition. It is reduced nearly

[36] T 2, 437.
[37] T 2, 438.
[38] T 2, 438.

or quite to zero when belongingness is absent . . . but it is there beyond question." [39] But it is not there beyond question. In the first place, we have been repeatedly told by Thorndike himself that repetition itself is impotent. It is difficult to comprehend by what reasoning Thorndike assumes that, when alone, repetition is impotent, yet if combined with something else it is potent, when the only evidence is the something else, Thorndike's own belongingness. So far as the facts go, the presumption ought to be, under the rules of logic, that repetition had nothing to do with learning, even when belongingness was present, and that belongingness is the factor that figures in the learning. Repetition is dragged in gratuitiously for the purpose of saving it when in terms of the facts which Thorndike himself presents, it is not efficacious.

Analogous confusions occur in Thorndike's account of Mrs. Hamilton's experiment in delayed feeding in animals. The fact that the reward has less 'effect,' the longer the delay, is interpreted to prove the law of effect. Again the belief follows from the original assumptions, not from the facts. There is a much simpler explanation than that which involves retroactive influence on connections, namely, the amount of insight permitted by the situation as a whole. In Köhler's experiment on apes it was found that if the stick lay on the floor in such a fashion that the animal could perceive it at the same time that he perceived the banana, the one was observed in relation to the other. But if the stick were at the opposite side of the cage, so that the animal's back was turned when he saw the stick, the relationship of 'tool' was not observed. Thus, a delay between reaching the goal and being fed precludes, in proportion to its length, the development of insight or of relational response. Moreover, the time interval by no means demands the concept of retroactive influence because responses are integrated *ahead*, toward a goal in the future. Closure is one of the conditioning factors. That which is now occurring depends upon what will occur later, namely, the state of equilibrium. Meanwhile, stimuli are responded to in terms of the organization toward

[39] T 2, 438 f.

this remote end. It is the condition of the action-pattern in the present, the extent to which it is integrated with respect to both the remote end and the occurring stimuli, that determines the degree of insight at the time. If the remote end has been reached without the reward the closure is sufficiently complete and the tension sufficiently resolved that the animal ignores the reward. In plain language, he fails to understand the relationship of the delayed reward in the total situation.

In discussing Yarborough's experiment again the simpler explanation is turned down in favor of a more complicated one. Figures are presented [40] demonstrating that the activity of an organism increases with nearness to the goal (increasing energy), but are twisted around to fit the assumptions of connectionism even in face of the admission that "the physiology of this may be mysterious but the fact seems sure." [41] There is nothing mysterious about the laws of dynamics, when it is self-evident that the nervous system is an energy system. Another difficulty, remarks Thorndike, "is to understand how a connection can be strengthened by any force." [42] True enough; energy does not behave that way. If it did we would have had connectionism in physics long ago.

When we begin with the assumption of a unified field, all points within it are 'connected' at all times; they always were and always will be so long as the system of energy in question exists. The 'connections' are only the fluid state of a field that follows laws of balance. A and B, as two experiences in this field, are increased structurizations of parts already related. Thus when the experiences emerge, they emerge already integrated. There is no forming of a connection; the relationship was there in the field, previously, and is there continuously. The same total situation which *figures* one part of the field, or sets up a tension, reduces the potential in the rest of the field in relation to it, and produces the differ-

[40] T 2, 453.
[41] T 2, 481.
[42] T 2, 482.

ential between the parts. It is all one act. Mechanistic principles require three separate acts, with no previously existing relationship between the parts to be related, to explain where the relationships come from. What system of thought could be more saturated with mysterious forces than the mechanistic theory, exemplified in psychology by connectionism?

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IN DEFENSE OF STIMULUS–RESPONSE PSYCHOLOGY

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Probably no psychologist would care to deny that the stimulus-response conception is somehow an essential feature of experimental psychology. Not only did the earliest physiological psychologists use the terms but they found it necessary to work with stimulus and response phenomena. They presented stimuli to their subjects, recording their responses either in terms of time (reaction time) or introspective observation reports (psychophysics). In the more recent experimental period the stimulus-response technique established itself as the *sine qua non* of all psychological work.

We might add too that the use of this concept continues a trend begun when the biological organism became a factor in the description of psychological phenomena. It must be observed further that the stimulus-response conception is essential alike to those psychologists who favor either the mentalistic or the behavioristic approach and interpretation. The only difference is that the former psychologists regard the response as psychobiological or psychophysical while the latter stress the physical or biological exclusively.

It is a bit paradoxical therefore that psychologists should assume a rebellious attitude toward the stimulus-response conception. To refer to only a few writers we find Woodworth (13) asserting that we must minimize the weight of the stimulus in psychological happenings. Thurstone (9) also proposes the dethronement of the stimulus as a much overworked factor in psychology. Most significant, however, are the remarks of Klüver (5) who while discussing animal work, ordinarily regarded as the strongest intrenchment of the stimulus-response conception, says that the stimulus-response formula is not only misleading and incorrect, but positively harmful.

In the case of both Woodworth and Thurstone the protest against stimuli is motivated by the interest in the dynamic character of the organism. Woodworth and Thurstone, stressing inner drives, want to make psychological phenomena more internal than the stimulus-response formula appears to them to allow. It is only fair to observe that both these writers are correct in objecting to a conception that makes of an individual a sheer weather-vane indiscriminately turned about by every wind of outside influence, and especially if the inner drives are regarded as concrete behavior developments, whether called motives, desires or interests. But this means only that they are protesting against a faulty conception of stimulus and not against the intrinsic stimulus conception [1] at all.

The writer plans to indicate also that what Klüver calls the fundamental mechanism which he thinks should replace the stimulus-response conception, is really a more adequate notion of stimulus-response rather than anything else.

Since we cannot dispense with these concepts the writer suggests that the unsettled condition with respect to them calls for a more adequate definition and understanding. Stimulus and response mean different things to different workers. Certainly the terms are not used with precision. It is to be expected that a concept or phenomenon understood in an objectionable way should be misprized. In the present paper we plan to examine briefly the use of the stimulus-response terminology with the hope that it will lead to a more satisfactory interpretation of stimulus-response phenomena.

The Stimulus

What is a stimulus? There are at least four answers to this question.

A. Object or Situation.—The adherents of the stimulus-response school of behaviorism regard the crude object or situation as the stimulus. When a stick is threateningly raised toward a dog the crouching is regarded as the response to the stick as a stimulus. Calling the stick as such a stimulus

[1] While it is customary to think of a stimulus and a response conception, there really is a single phenomenon involved.

can obviously be nothing more than a first approximation. We might grant too that so doing was a means whereby the behaviorist attempted to overcome the mentalistic attitude which he considered as wrong. This interpretation then may be regarded primarily as methodological rather than strictly descriptive.

Now it requires no argument to see that the crouching reaction is also performed when the hand alone is raised as the threatening gesture and when the dog is involved in other situations. When an object is regarded as a stimulus Klüver is quite right in asserting that there is no definite way of correlating responses and stimuli. Objects or situations can only be regarded as sources of stimulation. It is necessary then to ascertain in a more refined way just what is the effective stimulating factor.

In a recent experiment on emotional excitement Kellogg (4) used among other stimuli a five-foot chicken snake which was placed on the subject's lap. The measure of excitement was the decrease in steadiness in reacting to a plate-and-stylus tester. The effect of the snake was not so great as was expected, and not so large as that produced by handling a brain, cutting a mouse in two, and being dropped in a backward-falling chair. Now here it is clear that we cannot regard an object as a stimulus constant and simply record the kind of reaction made to it. Rather, the experimental question is what kind of stimulus function resides in the object for a particular individual.

B. Physiological Stimulus.—In the first stimulus conception in modern psychology, physiological stimuli such as light rays, air waves, etc., were regarded as arousing psychological reactions. There are several objections here. To begin with, if light rays, for example, are regarded as objects they are just as impotent as other objects to supply us with any discriminatory analysis of psychological happenings. There is no exact correlation of stimuli and discrimination responses. Purple discrimination is not correlated with a single wave length. Orange discrimination can be correlated with either a single wave length or with a combination of two wave lengths. With complex colors the difficulties multiply.

Again, on the basis of physiological stimuli psychological happenings are reduced to the level of simple discrimination. The entire rich field of human behavior is left untouched. Whether or not it would be theoretically possible to reduce all complex objects to simple physiological stimuli it is certain that on such a basis an elaborate human psychology would be inconceivable.

To many psychologists the most serious objection to making physiological stimuli into psychological stimuli is that the objective character of the stimuli is set aside. It is inevitable that to think of light rays as stimuli leads to a mentalistic position. Light rays are really correlates of mentalistic qualities and once we accept the light ray as a stimulus the psychic correlate must be brought into the picture. From the organismic standpoint it is a peculiarity of psychological history that the behaviorists accepted this stimulus conception from the structural psychologists. It is surely an obvious subterfuge that the response correlate was made into a receptor action by the behaviorist, since to do so means both leaving out the qualities altogether as well as reducing a psychological phenomenon to the physiological operation of an end-organ.

To take over the physiological-stimulus conception means also that psychological stimuli are reduced to nothing more than conditions which set physiological structures into function. Hence, on the basis of the physiological conception of a stimulus, which is eminently satisfactory for physiology, psychological phenomena lose their adequate description. For the psychological reaction becomes nothing but physiological action indifferently set off by vague conditions.

C. Stimulus as Occasion.—All those writers who look upon stimuli as merely occasions for responses, or who regard stimuli as mere energy releasers, think in terms of simple causal relations. They prefer to put the causes of psychological reactions into inner processes in the individual rather than into external causes. This conception takes two forms, neither of which really touches the stimulus situation.

The first of these views, which makes the individual act

because of drives, has two aspects. It is assumed that these drives are powers of some sort. In this form stimuli are not dealt with at all, because the whole conception is out of line altogether with concrete activities subject to definite observation and experimentation. When drives are regarded as actual responses they must be considered as configurations of behavior which the individual has built up through stimulus-response interactions. When drives are looked upon as actual desires, interests, and purposes, stimuli take on great importance. They operate very powerfully in the development of psychological phenomena. It is only after such response equipments are built up that a less powerful operation of their stimuli may later call out the reactions so built up.

The second form of the stimulus-occasion conception places the activating influences of psychological phenomena in the functioning of the biological structures. Hunger and thirst stimulate eating and drinking. Here it is questionable whether it is better to look upon hunger and thirst as drives rather than stimuli. If it does not seem better to do so there is of course no objection made to stimuli and there is no reason why they should be regarded as mere occasions for action. We must point out that hunger and thirst only seem to support the occasional view of stimuli because they happen to be periodic conditions. It is a very pertinent question to ask whether there are enough varied physiological conditions to be generally thought of as drives for the enormous number of complex activities that psychological organisms perform.

D. Stimuli as Organismic Functions.—In opposition to all of the above views stands the organismic position which looks upon the stimulus as a functional affair analyzed out of a psychological situation. The fundamental assumption is made that there are two essential variables for the description of psychological phenomena. Corresponding to the movements or actions of the responding organism, is an action or function performed by the stimulus object with which the organism is in interaction. In support of the organismic view of stimuli we may mention that it is found implied in some

of the most widespread historical psychological positions. For instance, the mentalistic psychologist who declares that perception is an interpretative process has already pointed out that the way an organism perceives an object depends upon the former interaction of the two. This undoubtedly can be interpreted to mean that some object, no matter what its physical characteristics may be, takes on a particular stimulus function which evokes a characteristic response to the object in question.

To the writer it seems that the fundamental contribution of the Gestalt movement lies precisely in the coördination of the functional effect of an object, several objects, or some situation, with a type of reaction. It is suggested, however, that what is happening is better interpreted as a stimulus-response mechanism than a phenomenological happening. Phenomenological concepts are too closely tied up with inscrutable subjective or mentalistic materials.

The student of psychology must also choose between the notion that actions are describable in terms of the stimulational functions of objects derived from the individual's contacts with those objects, and the view that objective patterns of things are simply correlated with brain patterns. This is another non-interactional conception implied in Gestalt doctrine.

We have elsewhere (3, ch. 2) discussed the extreme functional character of stimuli. The experimental problem of stimuli then is to discover what response is integrated with what stimulational function. What sort of stimulational function we are dealing with may be determined through the discovery whether a given reaction is aroused by a particular object, one of its qualities or properties, or the relation between several objects or properties. Still another question here is how long a particular stimulus function will operate to elicit a certain response. Klüver has made this point in his reference to the fragility of a behavior mechanism, though he seems to think of mechanism not as an integration of a behavior configuration or function, with a stimulus function, but exclusively as some condition in the organism. It may

be added here that the basis for the fragility of the integration of stimulus and response functions must be sought for in the conditions surrounding the organism and the stimulus objects. These conditions might be located either in the individual (injuries, interest, fatigue, satiation, etc.), in the object (qualities, intensity, size, etc.), or in both (distraction of various sorts).

The Response

With respect to the response there are also various conceptions, five of which we isolate for discussion.

A. Response as Mental Reaction.—Historically first is the conception of mental responses. This is the familiar notion that a mental process or reaction is excited when a light wave of a certain frequency stimulates the eye and sets up a physiological process terminating in the brain. Probably the most recent variation of the mental response conception is the conversion of the mental process of Act Psychology into the phenomenological factor of Gestalt Psychology. It is hardly necessary to review here the objections to this conception. We may, however, suggest that it requires at least such an emendation as Warren's (10) Double-Aspect hypothesis to bring it within the range of objective theory.

B. Response as Neural Process.—One of the earliest attempts to make objective the data of psychology was to look upon the neural function as the response. From an observational standpoint it is clear that this inevitably makes the response consist of part of a larger happening. No doubt the various researches on neural extirpation and other recent experiments constitute a basis for the criticism of this theory.

C. Response as Muscular or Glandular Action.—Warren (11) as a critic of what he calls radical behavioristic psychology has pointed out the insufficiency of muscle or glandular action for psychological description. Incidentally his criticism throws into relief the conception of response as an end process in a neuro-muscular event. This conception, like the preceding, limits the whole response to a very small part of it.

D. Response as Complete Body Action.—Many psychologists have been impressed with the criticisms of response as partial bodily action whether neural or muscular and have therefore regarded response as a total neuro-musculo-glandular configuration. In support of this conception two remarks may be made. In the first place, no complaint can be made as to the objectivity of this conception. It undoubtedly does not go beyond any sort of observable phenomenon. In the second place, it cannot be criticized for leaving out any feature of a biological occurrence. Nevertheless, we cannot but be impressed with the incompleteness of describing a psychological act as a sheer biological phenomenon. Granted that we may describe completely in neural, muscular, and glandular terms some movement or posture of an organism when it is running, is this sufficient? Does such a description really tell us what is going on? Let us recall the old controversy with respect to the James-Lange theory. James's critics not only required a mental state in the description but wanted it to precede any organic happening in order to be able to interpret the running as a fear reaction. Otherwise, they said, the psychological description was incomplete and inconclusive. We may agree that the postural description is insufficient but this does not mean that a mental state is necessary. Rather, what is required is adequate information concerning the stimulating process. This brings us then to the interactional conception of response.

E. Response as a Phase of Psychological Interaction.—In the writer's opinion a response can only be satisfactorily described as a factor of a stimulus-response interaction. The point can be especially well illustrated by speech behavior. We observe an individual saying 'yes.' Now what is the response here? We can with suitable techniques describe almost perfectly this entire happening as an anatomical and physiological phenomenon. But still we know nothing in any fundamentally psychological way. We do not even know whether the person agrees or disagrees. To know that we must also know about the stimulus. If the stimulus function is localizable in the words "Are you going with us?"

we may take it to be a response of agreement. If on the other hand the person has been asked, "You refuse to go with us?" his "Yes" is an entirely different kind of response.

Attention is called to the fact that our psychological descriptions of responses are inadequately given in terms of crude performance. The ascertainment of the nature of the crude response is only the beginning of our knowledge as to what a response actually is. A response is only one of two mutually necessary factors of the stimulus-response interaction. The response is an adjustmental function of the organism, a dynamic factor in a complex situation and not merely a fixed and static configuration of bodily activity. Several writers (3, 12) have pointed out that in human behavior, at least, there are all kinds of complicated social situations to be taken into account when responses are described.

The Equivalence of Stimuli

If stimuli and responses are both dynamic functions in psychological phenomena (behavior segments) there must be an exact correlation between them. This hypothesis has apparently been recently challenged by those psychologists who use the conception of equivalent stimuli. There is a sharp conflict between these two viewpoints. It appears clear that the notion of equivalent stimuli is based upon the conception that stimuli are objects. The writer submits that the splendid experimental work in the field of equivalent stimulation done by the Gestaltists generally (6), Lashley (7), Klüver (5), and the Leepers (8) does not, as Klüver seems to think, upset stimulus-response psychology but strengthens and supports it with definite experimental props. It is precisely such work that results in the final ascertainment of what objects, combinations of objects, relations between objects, or phases of objects carry the stimulus function for a particular response.

Some interesting questions bearing upon the stimulus-response problem are suggested by the results obtained by the Leepers (8) in their study of equivalent stimulation in

human learning. These writers were interested to prove that learning involves large, dynamic, neural patterns rather than specific, neural connections. Accordingly they presented one group of their subjects with varying stimulational materials (mazes, melodies, rhythms) and the other with stimulus objects which were not varied. They found that the rate of learning for the varied or unvaried material was roughly the same.

Now from the standpoint of the stimulus-response problem it is significant that these writers remark that the internal patterns of the stimuli must remain the same throughout the variational limits. It may be, therefore, that the internal pattern and that alone carries the stimulus function. If this is the case, then the stimuli in our estimation were not equivalent but identical. The objects (presented materials) in part of which the stimuli inhered may be regarded as equivalent.[2]

Again, these writers found it necessary to distinguish between habit and sensory equivalence. The former is indicated by the words house and dwelling. The Leepers wished to avoid presenting materials that would already have responses connected with them. In such a case they thought there would be no stimulational equivalence. What they call sensory stimulational equivalence would be similarly functioning variations in presented materials, without such verbal habit or other reactional complications. Here the question arises whether it is possible to isolate genuine stimuli (functions) without having functionally related responses connected with them. Sensorially equivalent stimuli in the sense of these writers are variable objects whose intrinsic stimulus and response connections have not been analyzed out, while habit-derived equivalent stimuli are those in which the stimulus-response connections obtrude themselves upon the observer.

[2] The present writer is not now interested in the neural question raised by the quoted authors. He accepts their conclusion though upon somewhat other grounds. But he does believe that a crucial psychological question here, calls for precision of interpretation.

The Problem of Behavior Distraction

Some recent remarks of Dulsky (2) concerning distractions are of interest in clarifying the interactional or organismic stimulus-response conception. This writer suggests that we should call a noise, for example, a distraction only when it exerts a deleterious effect upon a performance. Such a suggestion appears logical and to the point. A certain noise is only a distracting noise when it distracts. In other words, the question when a noise of given intensity and quality-mixture is a distracting stimulus depends upon when it functions to call out a distraction response. This suggestion fits in admirably with the interactional conception.

The phenomenon of distraction can help us further to clarify the stimulus-response problem. Consider now the general fragility of the distracting stimulus-response mechanism. This fragility is referred to in the assertion that there is no object which inherently has a distracting power. This assertion in turn is based upon the familiar fact that the distraction effect of noise upon performances is extremely relative. In the case of reaction time Cassel and Dallenbach (1) point out that "most frequently the 'distraction' serves to lengthen the time; but sometimes it decreases it; and occasionally, after a brief initial disturbance, it leaves the time unaffected." This type of experimental set-up throws into relief the competitive character of stimulus-response events. There are many factors that must be assiduously searched out to discover why the stimulus function of a particular stimulus object, rather than that of another, operates.

In the experimental set-up in which is studied the distraction effect upon the efficiency of a person over a period, there are two possible analyses.

First, the question may be, Will the distraction function of a certain noise operate or not in view of the person being engaged in an interesting or necessary interaction with a series of task stimuli? Here the distracting stimuli unless extremely intense may be regarded as not having an equal chance as in the reaction time set-up. Also, we may regard

this situation as one in which a stimulus function has to come into action not in competition with another specific stimulus function but with a social form of interaction, that is to say, not with a specific stimulus-response interaction, but with a series of undetermined stimulus-response activities.

In the second place, what are called distractors are not always stimuli but rather constant or intermittent settings of stimulus-response situations. The settings will be effective in breaking up stimulus-response action or not, depending upon the degree of the person's interest in his performances, as well as upon the intensity of the sound or other distracting circumstance.

Are there any objects or situations in which there inhere any absolute distracting stimulus functions? Yes. Without a doubt a burning match applied to a normal person's face will call out a distraction response. This stimulus function will inevitably elicit a distinctive movement or posture. The writer submits that all of these considerations concerning the distraction phenomenon emphasize the interactional feature of stimuli and responses.

To summarize, the writer offers the proposition that in view of the experimental history of psychology it is probably inevitable that psychologists should work with stimulus-response conceptions. A number of these have been briefly examined and the conclusion reached that there are obvious advantages and disadvantages in stimulus-response conceptions. Furthermore, the writer proposes that those experimental studies which throw into relief the stimulus-response problems, even when the workers themselves think they are detrimental to stimulus-response psychology, are on the contrary aids to a more effective determination of stimulus-response phenomena, and incidentally support stimulus-response psychology.

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THE EXPERIMENTAL SITUATION AS A PSYCHOLOGICAL PROBLEM

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Though it is a well-known fact that experimentation in human psychology presents serious difficulties, little has ever been done to account for this fact. One finds glib explanations to the effect that psychology deals with very complex phenomena, but scarcely any effort has been made to probe more deeply. It would seem, however, that the task is worth undertaking, for a close analysis of the peculiar difficulties of the psychological experiment should go a long way towards their solution. Herein lies the purpose of this paper.

Analysis discloses that the experimental situation in psychology is itself a psychological problem. Because one is obliged to study psychological phenomena in an intact conscious organism that is part and parcel of a social environment, the isolation of factors is difficult from the standpoint of experimental procedure just as it is dangerous from the theoretical standpoint.

We may begin the elucidation of these statements by asking what factors are necessarily present in any scientific experiment. If we take chemistry as our prototype, the answer may be given in the following schema:

A. Experimental materials, *e.g.*, chemicals.
B. The experimenter, *e.g.*, a chemist, who has functions as follows:
    1. To select, prepare and arrange the materials of *A*.
    2. To observe the reactions in the resultant mixture.
    3. To record what is thus observed.
    4. To interpret the records.
C. Apparatus, *e.g.*, scales, to assist the experimenter in carrying out his functions.

In the ideal experiment every factor in the above schema operates autonomously, i.e., as a separate and distinct entity. Every factor stays within its own domain.[1] But we may at once note certain important respects in which psychological experimentation honors this ideal arrangement in the breach. In this discussion we shall use the terms 'experimenter' (Er) and 'experimentee' (Ee) to refer to the person directing the experiment and the one who receives the instructions, respectively. Sometimes the Ee will be more specifically designated, according to his function, either as an introspective *observer* or a behaving *subject.*[2]

I. There is a type of psychological experiment—the introspective—in which the office *B*–2 (observer) belongs not to the Er but to the Ee. This is inevitable because in this type of experiment conscious experience is being studied and this can be observed only by the one in whom it occurs. At the same time that the Ee is *B*–2, however, he is also part of *A* (experimental materials), because he reacts to stimuli in addition to reporting his observations of these reactions. Since chemists, for example, do not study conscious experience as such, they encounter no such difficulty. The fact that the Ee is both *A* and *B* involves complications, as we shall see.

II. There is a type of psychological experiment—the behavioral—in which the Ee is expected to be and remain *A* (experimental materials) but by virtue of the possible self-conscious control of his reactions, he often assumes the rôle of *B*. As *B* he may introduce *A* materials (usually motives) without the Er's knowledge and thus vitiate the experimental results. Such a situation is impossible in chemistry, for example, for chemicals have no power of self-determination. The difficulty in psychology, on the other hand, is that every-

[1] The 'ideal experiment' is obviously an abstraction. Even in the physical sciences this ideal is seldom achieved, though it may be more closely approximated there than in psychology—as we shall soon show.

[2] The word 'experimentee' is the exact equivalent of the German 'Versuchsperson.' It is a general designation that may be used to refer to the person, whatever his function (that of observer or that of subject), working in coöperation with, but in a complementary relation to, the experimenter. It is an authorized English word and it is suggested that it be more commonly employed. *Cf.* in this connection the controversy between M. Bentley (3, 4) and J. F. Dashiell (7, 8).

one is a psychologist, so that the offices of *A* and *B* again run together.

III. The Er is a part of *A* (experimental materials) in many cases. To the chemical materials the personality of the chemist is of no conscious significance and in no way affects the reaction, but in a psychological experiment the Er may be as much a stimulus or determinant as other things. That is to say that in psychological experimentation the social environment can never be neglected and the Er is usually a part of the social environment of the Ee. The fact that the Er thus partakes of the offices of both *A* and *B* again raises difficulties.[3]

We shall discuss each of these three general peculiarities of the psychological experiment in turn. Our discussion will be based on experimentally established facts in so far as possible, but it may be stated here that nearly all of the points we shall consider ought to be further investigated as basic psychological problems.

I. Errors of Observational Attitude

Considering the first peculiarity of psychological experimentation, let us suppose a typical introspective experiment in the Titchenerian tradition and ask what are the factors in it that correspond to those in the above schematic list. It is obvious that the Ee in such a psychological experiment has a rôle corresponding in part to that of *A* (experimental materials) in the above, whereas the experimenter has a rôle corresponding roughly to *B* (experimenter). There are, however, some important qualifications. The psychological Ee is less than *A*. He is only one part of the total materials, only one or more of the total number of reagents. In addition there are weights, lights, instructions, and so forth, to which he reacts. But he is, on the other hand, not limited to *A*. He is also an 'observer,' which is to say that he has one of the functions of the chemical Er, *viz.*, *B*–2. Because conscious phenomena can be observed only by the one in whom

[3] At times, of course, experimental difficulties analogous to those just described arise in the physical sciences, as, *e.g.*, when the chemist must reckon with the heat of his own body and when physicists are obliged to posit a principle of indeterminacy.

they occur, the psychological Er is obliged to entrust one of the very important functions of the Er in chemistry, say, to the Ee. Hence the Ee in introspective experimentation of the Titchenerian sort is styled an 'observer.' This peculiarity of introspective work makes the control of experimental conditions very difficult, for there is a certain interaction of the observational attitude adopted and the development of the experience to be observed, due to the fact that the Ee is both the observer and the observed.

The effect of the attitude of the observer in introspective experimentation has long been realized. In addition to the well-known work of Külpe (12) on abstraction and that of Fernberger (10) on attitude in the psychophysical methods, we may note the recent studies of Anderson (2) and Wells (17). From these and other investigations it appears that under different attitudes of observation conscious experience either develops or is, at least, reported differently.

It is easy to deduce from this that an Er can correctly interpret introspective observations only if he knows under what attitude these observations have been made. But the Er can have such knowledge only if the Ee is able to adopt and consistently hold a determinate observational attitude. When the Ee fails to adopt and consistently hold the attitude expected by the Er, errors creep into the experimental results—'errors of observational attitude.' To avoid such errors the Ee must be trained in introspective observation.

In the light of this analysis it is not difficult to see what Titchener (15) meant by the 'stimulus-error.' This error arises whenever the Ee, after presumably reacting properly as *A* (subject), fails to perform function *B*–2 (observer) as he should. The error consists in failure simply to observe what the subjective reaction is, or, if one wishes to take the human brain as an instrument that registers reactions, failure to read the instrument accurately and to report only what has been *directly* read.

The following excerpts give Titchener's own description of the stimulus-error: "We live so habitually in a world of objects, and we think so habitually in terms of common sense,

that it is difficult for us to take up the psychological standpoint towards intensity of sensation, and to look at consciousness as it is, apart from any objective reference" (**15**, p. 202). ". . . one very dangerous source of error, in experiments upon the comparison of supraliminal sense-distances, is that the observer tends to judge, not in terms of sensation, but in terms of stimulus. He thinks not of the light-sensations, but of the grey papers; not of the sounds heard, but of the heights from which the balls must have fallen to give those sounds . . . this error . . . is known technically as the stimulus-error" (**15**, p. 218). "In some cases, the stimulus-error is ruled out by the observer's ignorance of the conditions under which the mental process laid before him for analysis is produced; in general, however, it can be overcome only by long training" (**15**, p. 350). The first of these passages tells us why the stimulus-error is commonly committed; the second, in what the error consists; the third, the ways in which it may be obviated.

As Boring (**6**) has pointed out, the stimulus-error raises a problem which must concern any psychologist, whether he be a Titchenerian or not. The mere possibility of a stimulus-error teaches us the importance of considering the control of observational attitude. It may be, as it indeed seems to certain psychologists to be the case, that the stimulus-attitude can yield quite satisfactory results. But whatever attitude may be designated for the introspective observer, that attitude must be adopted and held consistently if univocal data are to be obtained. It is only after long training that the Ee can ordinarily satisfy this requirement.

II. Errors of Motivational Attitude

We turn now to the second peculiarity of psychological experimentation. This consists in the fact that the subject may, as a result of the self-conscious control of his reactions, surreptitiously take over certain functions of *B* belonging rightfully to the Er. By assuming a critical and self-determining attitude instead of preserving the naïve and receptive orientation proper to a subject, the Ee introduces

extraneous factors, usually motivational, into the experimental situation and thus limits the Er's controlling influence. This difficulty cannot arise in such a science as chemistry, for chemicals are completely amenable to the will of the Er; they do not have 'minds of their own.'

But when one works with human materials one must reckon with the fact that everyone is a psychologist. How many subjects in a psychological experiment are purely receptive? How many are willing fully to adopt the humble rôle of subject in an investigation of their motives, aims and thoughts? Most, as a matter of fact, are carrying on a train of psychological activity that is rather about the experiment than a part of it by intention of the Er. "Where did I see that man before?—What is he getting at anyhow?—I wonder if he will ask me about this?—I won't tell him about that.—Could H. have been here for the same test?—How stupid that experimenter looks!—What a loud necktie!—How stupid he must think I am!—When will this be over?"

Such secondary reactions to uncontrolled stimuli and determinants mean that some motive in addition to those on which the Er is counting has entered into the experimental situation. The Ee has not remained a passive subject, part of *A*. He has outstepped his proper province, gone into *B*, as *B*–2 (observer) examined his reactions and perhaps as *B*–4 (interpreter) passed upon them, and then as *B*–1 (selector, etc., of materials) has introduced motivational attitudes (factors of *A*) on which the Er could not have counted but which nevertheless modify the experimental results.[4] Moreover, *the Ee is himself often unaware of the insidious ways in which these extraneous motivational factors have crept into and influenced the experiment.*

What he usually is aware of and probably could report is that he is critically scrutinizing himself and the Er. A

[4] It should be noted that such influences as the emotional predisposition of the Ee are not to be classed as errors of motivational attitude even though they may embarrass the Er just as much. We are considering in this paper only such errors as arise in the experimental situation itself and result from the interaction, peculiar to psychology, of the factors in this situation. Such influences as emotional predisposition are not of this type.

subject who acts in this way commits what we shall call the 'opinion-error': he entertains opinions about the experiment—what its purpose is and what he may reveal in it—instead of simply reacting in a naïve manner. The causes of the opinion-error are usually certain motives, such as curiosity and pride, in the interest of which the critical attitude is assumed. The consequence of this attitude is that, in keeping with the motives originally responsible for the opinion-error or as a result of new motives resulting from this error, the reactions of the subject are extraneously conditioned instead of being determined as the Er intended. When extraneous motives thus enter the experiment we shall say that the Ee has committed an 'error of motivational attitude.' It should be clear that the opinion-error and the error of motivational attitude as just defined are very closely related, so closely, in fact, that they may be regarded as two aspects of the same thing. Theoretically the opinion-error is preceded by and followed by elements of the error of motivational attitude, but in actual experience such a division is dubious. We shall accordingly not attempt to keep these two types of error separate in the following discussion but shall consider them together.[5]

To mention a few examples of these errors from experiments performed by the present writer, one Ee said after a session that included a test of his intelligence: "I didn't want to appear eager or excited. I wanted to appear indifferent about the results." Another, who had rated himself 5 on a scale ranging from 1–7 as to the extent to which he wanted to excel in the test, explained later: "It is often hard to distinguish between what your attitude really was and what you wanted it to be—even when you want to be honest about the matter. I rated myself 5 before but now I should say it was nearer 7. I was trying to give the impression, I think, that I really didn't try hard and that that was why my score

[5] The term 'opinion-error' might be supposed to include the stimulus-error since the latter is, in a sense, a matter of *opinions* derived from stimulus cues and may in certain cases be obviated by keeping the observer from obtaining such cues. As here defined, however, the opinion-error and the precautions against it are, for the most part, matters of motivation.

was so low." Or take the case of the precocious child of six who is left alone in a room to choose for repetition between two puzzles on one of which he has previously failed and on the other of which he has succeeded. From outside, the Er observes that the child has chosen the puzzle on which he had previously succeeded but is not able to put it together again. The Er enters and asks casually why this puzzle was chosen. The child begins to fidget in an embarrassed way and says that he has made a mistake: he had intended to choose the other—the one on which he had failed. It is, however, quite clear from the situation and ensuing conversation that he had chosen as he had intended but that he now has a sense of having chosen wrongly because he had chosen the easier; moreover, this sense is sharpened by the fact of failure on the previously successful puzzle and by the interrogation of the Er. Again, there is the young man who is doing badly on a test and leaves off to deliver himself of the opinion that the experiment is hardly scientific because it is badly controlled and can show nothing. And another comes to the conclusion that the puzzles he has been asked to do are unsolvable—because he is unable to solve them. Finally, take the case of the Ee who says he has liked better certain puzzles on which he failed than certain others on which he succeeded. Interrogation soon shows that he reports this because, in his opinion, only a weak character would prefer easy to hard things. He is therefore telling the Er not what he did like but what he thinks he ought to have liked.

The manner in which pride operates as an extraneous motive in the rôle of the subject is illustrated in nearly all of these examples. Pride causes the Ee to reflect about his behavior in the experimental situation and then to modify it in accordance with certain opinions that arise in his mind as a result of these reflections. The object of such modifications is usually to save the face of the subject. He naturally wants not to appear stupid or ill-bred or greedy or immoral, and so forth. Sometimes the mere revelation of a need, whatever its nature, is held to be wrong and behavior is guided accordingly.

The modifications of behavior that result are usually a form of self-justification. The experiment is badly performed; the test is unfair; the puzzles are unsolvable, and so forth. And not only do these reflections arise in the mind of the Ee and find verbal expression in some instances, but they modify his reactions in incalculable ways. Moreover, it is not simply his verbal reports that are modified but all his behavior—the speed of his bodily movements, the expression of his face, and the like.

Sometimes it is not pride but compliance that underlies errors of motivational attitude and the opinion-error. The aim of the Ee is then not to preserve his own self-respect but to save the Er's. Such cases occur when the Ee is a very suggestible person, eager to please the Er whom he respects. In a sense the proud Ee's we have previously discussed are negatively suggestible, or at least egocentric, and this sometimes makes itself manifest in a desire to influence the experiment to the dissatisfaction of the Er as well as to the preservation of their own inviolacy.

Such extraneous motivation usually derives from the social aspects of the experimental situation. Claude Bernard has pointed out that "In experimentation on inorganic bodies, we need take account of only one environment, the external cosmic environment; while in the higher living animals, at least two environments must be considered, the external or extra-organic environment and the internal or intra-organic environment" (**5**, p. 63). To this we may add that in experimenting on mental processes the social, the human environment becomes of crucial importance. And since this social environment in the course of experience becomes internalized in the form of standards, ideals, and taboos, it is necessary to consider also the endopsychic equivalent of the social environment.

One of the less often discussed but more generally important contributions of psychoanalysis to general psychology is the appreciation of the factors operative in the social situation. Common sense has long understood the methods of social intercourse—tact, subterfuge, amenity and con-

fidence—but only on the advent of psychoanalysis, with its investigation of the phenomena of transference and its development of the method of free association, has the science of psychology treated this problem with any success. From this work of the psychoanalysts experimental psychologists may learn the importance of a naïve attitude on the part of the subject (1, pp. 40–54; 10, esp. p. 45).

The naïveté of the experimental subject, *i.e., his readiness to accept determinants from the Er and react uncritically*, may be graded as follows:

A. The subject does not even know that an experiment is being performed on him.

B. The subject knows that an experiment is being performed on him but he does not know its object.

C. The subject knows both that an experiment is being performed on him and what its object is.

It is obvious that the naïveté of grade *A* is the most desirable and, as we shall see, to attain this is not an impossibility in certain cases. We shall now consider ways and means of approaching this ideal.

(1) The use of children and unsophisticated adults is to be recommended.[6] The very young child is quite certain to be less critical of his own behavior in the experiment or to consider what that behavior is revealing to the Er. A rather striking example of this is from a five-year old boy who was asked to solve some puzzles. "Can't they put them together themselves?" he queried. Unsophisticated adults—by which is meant adults who are not well educated, perhaps even below normal in intelligence—would also be satisfactory subjects in certain cases.

(2) The use of stimuli or determinants which, because of their nature or strength, arouse the subject in a normal way almost in spite of himself are often helpful. There are certain responses, such as withdrawal from a pain stimulus, that the Ee cannot inhibit or modify very much. It is not always possible to employ stimuli or determinants of such prepotent

[6] It is part of the Er's duty to decide by the best available knowledge when to take or not to take a given precaution.

character or strength, but those that are used should approach this goal as closely as is consistent with the other experimental conditions. This implies, too, that the bodily type of response is apt to be more dependable than verbal reports. Words are too easily swallowed or modified on the way from larynx to lips.[7]

(3) An expedient that is of the greatest assistance in many cases is to observe the subject from a secret outlook. It is easy to understand that individuals will act much more freely when they are (or think they are) unobserved. They are then off their guard and their behavior accordingly conforms much more closely to the pattern of their impulses than when they are under scrutiny. This expedient is of great importance because it practically transforms the experimental situation from a social into a solitary one and, as we saw earlier, it is the social character of the experiment that raises the problem of the opinion-error.

(4) The subject should be kept as far as possible in ignorance of the true object and technique of the experiment. That the subject usually looks for the purpose of the experiment, to be guided accordingly, is shown by numerous instances in the experiments of the present writer. What is more, the subject knows as well as the Er the evil consequences of this tendency, as is well exemplified by the Ee who said in informal conversation after an experiment: "At first I tried to figure out what you were getting at but then I realized this would be unfair. I tried to act from then on as a subject ought to act."

Venn (**16**, pp. 345, 347) long ago pointed out the need for the precaution we are considering, though in a different context—a discussion of the logic of chance. An experimental instance of the effects of the foreknowledge of the subject is furnished by some work of Stumberg (**13**). She had two groups of subjects, one sophisticated, the other naïve. The experiment was the well-known one involving

[7] *Cf.* in this connection the discussion of Thurstone (14) in which he says in part: "It must be recognized that there is a discrepancy, some error of measurement as it were, between the opinion or overt action that we use as an index and the attitude that we infer from such an index" (14, p. 262).

the use of word-association for the detection of criminal acts. One group of subjects knew the way in which the test should operate and here the criminal was practically never detectable. The other group knew nothing about the test and revealed its guilt consistently. The inference is that if the subject knows what it is that may be revealed by his behavior, his need for inviolacy is able to operate directly in behalf of his self-respect. Forewarned is forearmed.

It is not only the object or purpose of the experiment that is to be kept from the Ee but he ought also to be kept, as far as possible, ignorant of its technique or *modus operandi.* Sometimes the purpose of the experiment is partly implicit in the technique, as in the case of a method the present writer has often employed—that of telling a subject that he is to be given an intelligence test when he is, as a matter of fact, simply being aroused emotionally by this determinant, the Er not being in the slightest degree interested in the measurement of intelligence and taking no stock in any results relating to this. If a subject should, in such a case, know the truth about the experimental technique, the experiment would obviously be impossible.

Some ways of keeping the Ee from knowing the true object and technique of the experiment may now be briefly mentioned.

(*a*) Mislead the subject by deliberately assigning a false object at the very outset of the experiment. This expedient is very dangerous, however, because the Ee is apt to be influenced as much by false as by true opinions of the object of the experiment. In applying this method one must therefore carefully select a false object consistent with the true object of the experiment.

It is well to give the subject some idea of what the experiment is designed to demonstrate so as to prevent him from making conjectures that will be uncontrolled. But in assigning an object for the experiment, it is well to avoid vagueness or ambiguity, for the effects of this sort of knowledge are more incalculable than are those of a definite and absolute belief. It is, therefore, better to lie outright in many cases than to

prevaricate. Sometimes, as in the case mentioned above, the lie in which an object is assigned to the experiment may serve to motivate the subject and this is ideal because his opinion about the purpose of his acts is then directly related to them in a causal way. Control of the subject's attitude is then maximally satisfactory.

The feelings of the Er in cases in which he employs the expedient of secretly observing the Ee or of lying to him are not always pleasant at first. The Er must overcome certain natural inhibitions against such procedures and this is sometimes difficult. He cannot, however, allow himself to be deterred by such scruples any more than the physiologist can be deterred by the arguments of anti-vivisectionists. Provided that the Er is convinced that his procedures will in no way seriously injure the Ee, he must solace himself with the consideration that the scientific end justifies the scientific means.[8]

(*b*) Keep one step ahead of the subject in sophistication as to the object of the experiment. There arise inevitably cases in which the subject knows the true object of the experiment as performed up to that time. In such cases his knowledge, if known to the Er, may be counted upon and an excellent control of preceding experimentation is thus sometimes obtained.

(*c*) Interrogate the subject after the experiment as to the opinions he entertained during its previous course. Find out if he knew anything about the experiment before he came into the laboratory.

(*d*) After the experiment, initiate the subject into the 'fellowship of research' by pledging him to secrecy. For example, we handed the subjects in one of our experiments a paper with the following words on it and, after they had read it, elicited from them a verbal promise to carry out its import.

[8] "The prejudices clinging to respect for corpses," writes Claude Bernard (5, pp. 101, 103), "long halted the progress of anatomy. In the same way, vivisection in all ages has met with prejudices and detractors. . . . It is impossible for men, judging facts by such different ideas, ever to agree; and as it is impossible to satisfy everybody, a man of science should attend only to the opinion of men of science who understand him, and should derive rules of conduct only from his own conscience."

"It is very important that you should not discuss in any way *anything* that you did in this room. We have to ask this of you so that we may keep the conditions of our test constant. I hope you will not even say that you had an intelligence test or that you did puzzles; and that if you are asked what you did, you will say that you were requested not to discuss the matter. Since the people who have been here already have probably not had the same test as yours but may have it at a later time, you should not say anything to them either." [9]

It is interesting to compare in passing the stimulus-error, which was so important in the psychology of Titchener, with the opinion-error, which is to us of much greater moment. For it is paradoxical that these two types of error should be obviated in such contrary ways—the former through the training, the latter through the naïveté of the Ee. The solution of the paradox lies in this: These two types of error usually arise in two quite different types of experiment—the one more strictly introspective, in which the Ee is an observer (*B*–2 in the above schema), the other more strictly behavioral, in which the Ee is a subject (*A* in the above schema). What constitutes a good Ee depends entirely upon the duty he has to perform in the experimental situation.[10]

Psychology, in so far as it has scientific pretensions, derives from two sources. There is, on the one hand, the epistemological tradition that goes back to English empiricism, Fechner and Wundt and that was most recently espoused in this country by Titchener. This approach is concerned with

[9] This precaution—like the others—is not wholly satisfactory. A subject who has been told by a previous participant that the object of the experiment is a secret thus obtains an attitude that may modify his performance in the experiment. Interrogation at the end of the experiment may afford some check as to the existence of such an attitude.

[10] It is to be noted that we have been considering the second peculiarity of psychological experimentation almost entirely as it relates to behavioral experiments, *i.e.*, experiments in which the Ee is simply a subject. It should not be overlooked, however, that even in such introspective work as was discussed in the previous section, the Ee is capable of errors of motivational attitude and the opinion-error and that these errors may influence observational attitude. To what extent and in what manner this actually occurs is yet to be investigated but that some influence of this type is possible is suggested by Boring's discussion of the relation of incentive to the stimulus-error (6, pp. 468–469).

the principles of human understanding—to follow Locke—and this ends in a study of the psychophysical relation with special reference to problems of sensation. These problems have been attacked by fairly adequate experimental techniques and the result has been called structural psychology. On the other hand, there is functional psychology, representing the biological envisagement of psychological problems and concerned primarily with the principles of motivated human behavior. This is a product of evolutionary biology, as sponsored by Darwin, Romanes, and others, and of medically grounded mental science, such as is found in the 'Principles' of William James and the teachings of French psychopathology. Until quite recently the biological approach to psychology had yielded few experimental results on human behavior. There were clinical studies of personality, mental tests, and animal experiments, but little controlled experimentation on human motivation had been done. The trend is, however, changing, so that we shall doubtless soon witness a great increase in work of this last type, and it is in connection with such work that our analysis of errors of motivational attitude and the opinion-error is of special significance.[11]

III. Errors of Personality Influence

We come finally to the third peculiarity of psychological experimentation. This consists in the fact that, since the Ee is a conscious human being, in the social situation of the experiment the Er himself may function as part of *A*, *i.e.*, as a determinant. This possibility exists both for introspective and behavioral experiments. If the Er's influence as a determinant is not controlled, difficulty naturally arises. In physics or chemistry no such obstacle to experimentation presents itself, for the personality and psychological behavior of the Er are of no significance to the reagents.

We may consider first the possible effect of the total given personality of the Er upon the Ee. Whether the Er is, for

[11] The use of statistical methods for coping with the uncontrolled variables in psychological work is, of course, very important. It should be noted, however, that dependence upon the retroactive control provided by statistics decreases if such initial precautions in experimentation as those here suggested are taken.

instance, a man or a woman, white or black, Jewish or Gentile, are factors that may make a difference to the attitude and reactions of the Ee. There are even changes in the personality of the same Er from hour to hour or day to day that may alter the experimental conditions to which different Ee's are subject. Whenever factors of this type *have been uncontrolled* [12] and have played a part in an experiment, 'errors of personality influence' may be said to have occurred. To obviate such errors it is helpful if the experiment can be designed so as to proceed without the Er's presence, but when this is not possible, the systematic variation of Er's with comparable groups of Ee's is a useful expedient.

An experimental example of the effect of the presence of an Er upon the reactions of the Ee is afforded by some work of Ekdahl (9). This investigator found that "The presence of an experimenter during a free-association experiment affects the performance of the subject in a manner that cannot consistently be predicted. . . . Measured quantitatively the presence of an experimenter seems to retard the association-time [as compared with results obtained with the Er absent] with the majority of subjects. . . . Qualitatively the differences do not lend themselves readily to measurement, but that they were present a study of the introspections unmistakably brings out" (9, pp. 306–307). These results illustrate the possibility of errors of personality influence.

In addition to errors of personality influence as just described, there are certain other errors that may result from the third peculiarity of psychological experimentation. These we shall designate by the name 'suggestion-error.' The suggestion-error arises whenever the Er by some specific but inadvertent act imparts to the Ee an unintended motivational or cognitive determinant.

It is not difficult to see how an unguarded word, nod or glance from the Er may have a suggestive significance of marked consequence to certain experimental results. Even in animal psychology the importance of such influences is recognized: the famous mathematical horses of Elberfeld, who

[12] There is, of course, no error when the Er by intention becomes a determinant.

were being given clues by their trainers without the latter's intending to do so, are a case in point. The sensitivity in this regard of human subjects is obviously even greater than that of animals and the Er, if present in the experimental room, must accordingly refrain again and again from the natural expression of anticipation, surprise, disgust, admiration, and so forth—a histrionic art of no little difficulty.

Summary.—In this paper we have considered three general ways in which experimentation in psychology differs from and is more difficult than similar work in the established experimental sciences such as chemistry. We have also suggested certain remedies for the difficulties we have described.

The first peculiarity of psychological experimentation consists in the fact that the experimenter is sometimes not the observer: when conscious experience is the object of study, the subject must be the observer. From this results the difficulty that in such cases there may be a disruptive interaction of the observational attitude of the experimentee and the development of the experience to be observed, owing to the fact that both have the same residence, *viz.*, the experimentee. Hence arise what we have called errors of observational attitude, a special case of which is the stimulus-error. To obviate such errors the experimentee in an introspective experiment should be trained to take and keep a determinate observational attitude.

The second peculiarity of psychological experimentation consists in the fact that certain of the experimental materials (the experimentee) may regulate their own reactions: they have minds of their own and are self-critical. From this results the difficulty that uncontrolled experimental materials, *viz.*, motives, may be brought into the experiment. Hence arise what we have called errors of motivational attitude and the opinion-error. To obviate such errors every available measure should be taken to keep the experimentee, especially in a behavioral experiment, as naïve as possible.

The third peculiarity of psychological experimentation consists in the fact that the experimenter may be a part of the experimental materials: certain of the latter (the experi-

mentee) may react to the personality of the experimenter. From this results the difficulty that again uncontrolled experimental materials, motivational or cognitive, may be brought into the experiment. Hence arise what we have called errors of personality influence and the suggestion-error. To obviate such errors experimenters should sometimes be systematically varied; whenever possible the experiment should be designed so as to proceed with the experimenter absent from the experimental room; and the experimenter should always skillfully control the expression of his own thoughts, sentiments and feelings.

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ASSOCIATION AS A FUNCTION OF TIME INTERVAL

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The most thorough discussion of association as a function of the time interval between the items to be associated is contained in a chapter of Robinson's (**22**) excellent treatise, *Association Theory Today*. In this chapter Robinson reviews the problem in the light of some of the experimental results and comes to the general conclusion that the law of contiguity, if it is to conform with the results of observation, must express the strength of association as a continuous function of the time interval between presentation of instigating and instigated items, and not as a 'qualitative' law asserting merely that association follows as the result of the contiguity of instigating and instigated items.

Association occurs, he argues, in cases in which the associated items are separated by an interval, and even occurs in some cases in which the instigating item follows the instigated after a short interval, though the association is less strong. This fact of remote association may be described in two different types of law. We may hold that association is 'really' always dependent on a coincidence or overlapping in time of the associated elements, and that remote association is always mediated by intervening items. Or we may state the law in a quantitative form with associative strength expressed as a function of the time interval between items, and so avoid any necessity for excusing the failure of the law to fit the facts.

Robinson prefers the latter course. The mediating items can not ordinarily be demonstrated, nor can they ordinarily be disproved. They remain a speculative excuse for remote association if we assume that association depends 'really' on coincidence in time.

After considering some of the results of experiments in

which time relations between items are a matter of record, Robinson proceeds to set up a graph (Fig. 1) showing the 'general form of the law of contiguity or time interval' indicating a maximum of associative strength when the items

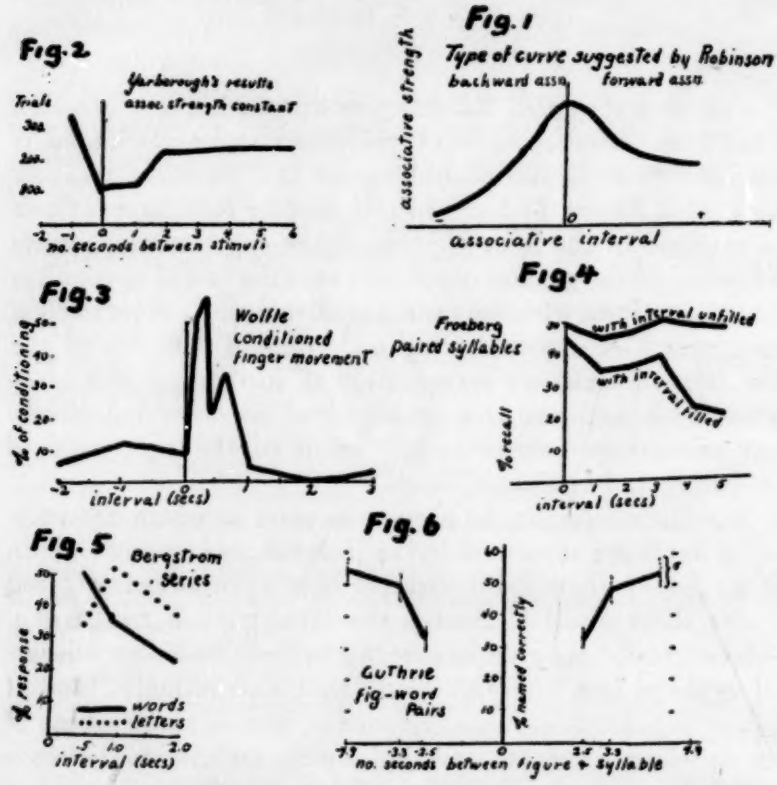

Figure 2 shows results with conditioning to the sound of a buzzer the response of a rat to electric shock. Learning was to the point of 90.9 to 92 percent of 'correct' response in the last 100 trials except for the interval — 1, in which case 340 trials produced only 75 percent of response. In the rest of the figures the ordinate is in each case associative strength expressed in the percent of associated responses elicited, and the abscissa is the interval between stimuli measured from the stimulus used as a cue with negative values for backward association.

are simultaneous, and diminished strength as the interval increases, with associative strength diminishing much more rapidly as the instigated item precedes the instigating by an increasing interval than as the instigated item follows the instigating by increasing interval.

The shape of this graph is based mainly on Yarborough's findings (Fig. 2) in an experiment (**29**) in which rats were trained to turn back in a maze on receiving an induction shock, and after this training a visual or auditory conditioner was introduced before or after the shock. Robinson makes no claim that his general curve does more than suggest the general type of the curves which will be found to hold, but he does say that "it does not seem at all unlikely that inflections of these types, though not always at these particular temporal points, will ultimately be shown to characterize analogous functions obtained under a wide variety of detailed conditions."

In short, Robinson would substitute for the somewhat dubious law of contiguity which states that association is dependent on coincidence of the associated items, a law which would state associative strength as a function of time interval, the curve with a maximum at the o value of the interval and descending more rapidly on the side of backward association than when association is forward.

To this conclusion of Robinson's there is a possible objection. An examination of the experimental material now available suggests that the characteristics of Robinson's curve based primarily on Yarborough's results, hold only for a very limited class of data and that according as the subjects are animal or human, or as the responses recorded are simple and brief, or complex and extended, curves which differ very materially from Robinson's will be discovered. We may find that the form of the curve is so dependent on the nature of the activity that possible mediating items offer the best explanation. Our choice may not lie between a law which states that association results only in cases of actual coincidence of the associated items, with mediating items not always demonstrable when the association is remote, and a law describing associative strength as a specific type of function of the degree of contiguity. The choice may actually lie between the former and a general statement that associative strength is a function of the degree of contiguity, but no function in particular.

Furthermore, an examination of the experimental material

may suggest that results depend on the form of associative mediation, that is, on the occurrence of mediating items. We may agree with Robinson that "the burden of proof should be assumed by those who insist that there is always a mediator, even when they are unable to demonstrate it, rather than by those who admit no mediation of this sort unless there is positive evidence." We may find, however, that some of the characteristics of the different types of curves obtained may be explained in terms of the type of mediation involved.

Experiments on the Time Factor in Conditioning

A review of the experimental work to date, in so far as it uses the conditioned response technique, is to be found in Wolfle's article (**27**). We may begin with Yarborough's pioneer work with the conditioning of rats to stimuli introduced before or after the original stimuli, an experiment which seems to have been suggested by the work of Carr and Freeman (**4**). Yarborough released rats into a runway at the end of which a right- or left-hand turn brought them to food. The course of the rat after making the choice was lengthened by baffles which made the run indirect. In 70 percent of the practice trials a slight electric shock was administered through a floor grid and the door at the end of the passage already entered was closed, forcing the animal to turn around and return to the point at which the paths separated. All animals learned to turn back when the shock was applied.

After this training a buzzer was sounded in connection with the shock. This buzzer was timed for some animals one second after, for some immediately after the shock, and for other animals 1, 2, 4, or 6 seconds before the shock. The first two groups, it will be noticed, were exposed to an opportunity for backward conditioning, and the others to forward conditioning.

The results are fairly well represented by Robinson's curve. Maximum learning occurred at the 0 interval which represented immediate succession. More trials were required for backward conditioning than for forward, but backward conditioning was present at the 0 and 1 second intervals.

Forward conditioning occurred even at the 6 second interval, but required more than twice as many trials to establish as for the 0 interval.

Cason (**6**), using one human subject, found it possible to condition a wink on the sound of a buzzer if the buzzer was sounded before the wink, but could not establish backward conditioning if the buzzer was sounded "when the natural wink was practically completed."

Switzer (**24**) also used the wink. With an electrically released hammer striking below the eye he attempted conditioning to the sound of a buzzer. With 18 out of 20 subjects backward association was established, the optimum time interval being from one-half to one second. Conditioning occurred in some cases with an interval of 2 seconds. Switzer also reports slight and unstable conditioning of the knee-jerk to the sound of a bell, the bell sounding "when the knee-jerk registered on the smoked paper."

Wolfle (**27**) conditioned the withdrawal of a finger from contact with a plate, the withdrawal occasioned by an induction shock, to the noise of a sound hammer. Towards the end of the 40 minute practice period, in which there were 200 pairings of the two stimuli, subjects were given the conditioned stimulus alone ten times. Associative strength was measured by the percent of instances in which the conditioned stimulus elicited the withdrawal. The average percent of conditioning in three practice periods at the various intervals used was:

Interval	−2	−1	−.6	−.2	0	.2	.3	.4	.6	1.0	2.0	3.0
Associative strength.	6%	12%	11%	10%	10%	51%	58%	23%	34%	5%	0%	3%

The graphic representation of these results (Fig. 3) differs from Robinson's proposed curve only in having a maximum at the .3 second interval (forward conditioning) instead of at the point of simultaneity.

Pavlov's *Conditioned Reflexes* (**19**) reports the experiments of Krestovnikov in which the conditioned stimulus was presented 5 to 10 seconds after the unconditioned, with negative results. Pavlov later reports (**20**) that backward conditioning

has been obtained with dogs, but that it is 'insignificant and evanescent' and on continuing the procedure the stimulus becomes inhibitory.

Schlosberg (23) found that with intervals of .20 and .44 second (forward conditioning) there was no significant difference in associative strength, but that below .11 second it was difficult to form a conditioned reflex.

Hilgard (15) found that intervals between 0 and .05 second were less effective in conditioning the wink than intervals between .05 and .40 second, the conditioned stimulus preceding the primary.

In general, experiments on the effect of time interval in conditioning seem to have distinctly consistent results. Backward conditioning is possible, but less effective than forward conditioning. With intervals beyond 2 seconds it is difficult to establish. The exception to the first statement is Cason's failure to establish backward conditioning of the 'natural' wink, which differs from the other experiments in that conditioning was attempted at a time when the response involved was practically completed.

Forward conditioning is possible up to an indefinite interval, the optimum interval lying somewhere between .2 and .6 second. In general the experimental results conform to Robinson's proposed curve, with the exception that the maximum lies not at the point of simultaneity, but at a point at which the new stimulus is separated from the established stimulus by a fraction of a second.

Time Interval in Association

Experiments in association, using nonsense syllables in pairs or in series, or syllables with visual patterns or other cues, are to be regarded as quite distinct from the experiments using the technique of conditioning. In such experiments the 'intent to learn' becomes an essential part of the event. Attention and sub-vocal speech play more important rôles. And the resulting generalizations are very different.

Backward association, as distinct from backward conditioning, has been shown to exist in some sense from the time

of Ebbinghaus's experiments with nonsense lists (**9**). Memorizing a list assists in memorizing it in the opposite direction. And memorizing a list makes it more easy to memorize a new list made up of items of the first which were not contiguous.

Backward association has been reported by Ebbinghaus, Müller and Pilzeker (**18**), by Cason (**8**), (who explains it in terms of uncontrolled forward practice), by Garrett and Hartmann (**12**), and by others. Remote forward association has also been established beyond doubt as a descriptive generalization. S. Froeberg (**11**), improving on the method of A. Wohlgemuth (**25, 26**), used five pairs of syllables presented simultaneously or with intervals of 0, 1, 2, 3, 4, and 5 seconds separating the members of each pair. For these various intervals he found 61, 49, 45, 48, 51, 49, and 49 percent of recall of the second syllable when the first was presented (Fig. 4). When the intervals were filled by reading irrelevant numerals these percents were 54, 45, 34, 36, 40, 24, and 22. With associated colors and letters he found successive association superior to simultaneous.

Bergstrom (**2**) had three subjects memorize 12 lists of 12 nonsense syllables on each of four days. Each syllable was exposed for .082 second and intervals of .302, .686, and 1.454 seconds between exposures were used. On a recall test the errors for these three intervals averaged 10.3, 8.9, and 7.5 respectively. With 30 subjects memorizing lists of 10 words and lists of 10 letters, and using intervals of .5, 1.0, and 2.0 seconds, the percents of errors on immediate written reproduction were 51.12, 36.52, and 23.9 for words, and 44.09, 52.65, and 38.44 for letters (Fig. 5). The optimum interval for the nonsense syllables was the longest interval used, 1.453 second. For the words the optimum interval was .5 second, the shortest used, and for letters, the middle interval, 1 second, gave best results.

The Writer's Experiment

To these reports may be added the results of an experiment recently completed in the University of Washington laboratory by Miss Dorothy Albaugh and the writer. Twenty nonsense syllables and 20 nonsense figures of equal areas were

made up into two sets of 10 pairs of syllables and figures. These were photographed on standard motion picture film. Each film was made up of five different orders of a set of 10 pairs. On two films the figures followed the syllables, and on two the syllables followed the figures, backward and forward association respectively.

These films were used with a projector operated by a synchronous motor and so devised that the exposure time and the interval between exposures could be adjusted independently.

Continuous presentation of the films was arranged by joining the ends of each film together. The subjects viewed the material in a direct projection screen which hid the projector. Each group of subjects was instructed that nonsense figures and nonsense syllables were to be shown (this was illustrated) and was then given seven repetitions of one of the series. The sixth and seventh trial presentations obviously repeated the order of the first and second.

When the practice was complete the subjects were instructed to write the names of the figures next shown, and the figures were exposed at so slow a rate that all subjects had an opportunity to write the corresponding syllable at their leisure.

Each frame was exposed for .41 second throughout the experiment, except for the final recall. Measuring the intervals between the beginning of the exposure of the first member of a pair and the beginning of the exposure of the second, the intervals used were 4.93, 3.33, and 2.55 seconds. Since the alternating current supplying the synchronous motor varied somewhat in frequency these intervals may have had an extreme variation of 2 to 3 percent. Between successive pairs a fairly uniform interval approximating 5 seconds was used.

Each group of subjects (averaging 12 persons) was given two lists to learn, one with forward association and one with backward association. Only one interval was used with any particular group. For each time interval four groups were used. These were offered the following sequences:

Group 1. Series A (word-figure) and Series B (figure-word)
Group 2. Series B (figure-word) and Series A (word-figure)
Group 3. Series B (word-figure) and Series A (figure-word)
Group 4. Series A (figure-word) and Series B (word-figure)

Each set of the twelve sets of subjects therefore was given one series for backward association and one for forward. One-half had backward association offered first and one-half had forward. One-half of the subjects had series A (which differed in its items from series B) presented first and one-half had B.

The subjects viewed the screen from a distance of 9 to 15 feet. One hundred and forty-five subjects were used. Twenty-four had all responses correct and 25 had no responses correct.

The results are summarized in the accompanying table (see Fig. 6).

TABLE I

Interval (seconds)	Forward Assn. Mean Number Recalled		Backward Assn. Mean Number Recalled		Difference		Diff./S.D.
		s.d.		*s.d.*		*s.d.*	
4.93	5.40	.42	5.48	.46	.08	.40	.2
3.33	4.92	.41	4.90	.45	.02	.52	.04
2.55	3.35	.32	3.38	.37	.03	.50	.06
All intervals combined....	4.57	.25	4.61	.25	.04	.28	.14

In the above table the results for forward and backward association are correlated, since each student was tried on each order. The correlations were .40 ± .08, .26 ± .09, .35 ± .09, and .37 ± .05 respectively. The standard deviations of the differences (column 7) take this into account.

TABLE II

SIGNIFICANCES OF THE DIFFERENCES BETWEEN RESULTS FOR DIFFERENT INTERVALS

Between Intervals	Difference	S.D.	D./S.D.
4.93 and 3.33.....	.48	.59	.81 (forward association)
3.33 and 2.55.....	1.57	.52	3.02 (forward association)
4.93 and 2.55.....	2.05	.52	3.94 (forward association)
4.93 and 3.33.....	.58	.64	.91 (backward association)
3.33 and 2.55.....	1.52	.58	2.62 (backward association)
4.93 and 2.55.....	2.10	.59	3.56 (backward association)

The items in the above table are not correlated.

From table I it is evident that the differences between forward and backward association are not significant, that the two methods of presentation were remarkably close in their effectiveness. This is not true for intervals differing in length. Between forward association with an interval of 4.93 and forward association with an interval of 2.55 seconds the difference in the average numbers of figures correctly named was 2.05, or 3.94 times its standard deviation, and between the same intervals in backward association the difference was 2.10 or 3.56 times its standard deviation.

In order to get some notion of the relative effectiveness of simultaneous association a new film was prepared with syllable and figure presented together. This represents simultaneous association only in the sense that the two items are presented simultaneously. They may not have been attended to simultaneously. And the task may have been quite different. The new series was exposed for the same time as the separate items in the other series, *i.e.* .41 second. The mean number of figures named correctly was 4.13, which may or may not indicate that the curve of Fig. 6 rises at the center point.

In general the outstanding results of the experiment were the close equality of associative strengths in forward and in backward association, and the increase in associative strength as the interval increased.

Summary and Conclusions

Experiments to determine the relation of associative strength to the time interval between associated items, when the conditioned reflex method is used, have given fairly consistent results. The curve is of the type described by Robinson, except that its maximum is not at the point of simultaneity but between .2 and .6 second in the direction of forward association. Experiments using association technique do not give such consistent results. Froeberg finds immediate succession (0 interval) to be more effective than intervals from 1 to 5 seconds. The present writer's results indicated the longer intervals both in forward and in backward conditioning as the most effective. Froeberg's subjects were instructed to avoid

verbalization, which precaution was not used by the writer because his subjects were naïve. The higher associative strength resulting from longer intervals may have been due to uncontrolled practice, and was undoubtedly related to intervening activities. Subjects reported often, as did Froeberg's, that they could "hold the first item in mind until the second appeared." It is the writer's opinion that the key to the differences in the results of experiments on association and conditioning lies in the nature of the intervening activity, and that it is highly probable that remote association in either direction must depend on mediating items and on the opportunity for simultaneous association offered by an overlapping of stimuli and of actions which serve as stimuli.

It is also the writer's opinion that one source of error in dealing with association has been the tendency to regard the stimuli or the stimulations as the associated items. A more proper view may hold that the association is between stimulus and response, and that the law of contiguity should be expressed in some such form as this: *Stimuli which are acting at the time of a response tend on their recurrence to evoke that response.*

The writer's interpretation of the results of the various experiments reported is that a search for a general law which will express associative strength as a function of associative interval is doomed to failure. A different function is apt to be found for each different experimental situation. It is indulging in speculation to propose that the underlying law must be stated in terms of actual coincidence of associated items, but there are some facts that incline us to that view. The fact that intervening behavior affects the result, that associative strength depends on the nature of the activity, and the fact that overlapping activities are entirely possible in every case with the exception of Cason's wink, which gave negative results, would all point to a necessity for actual coincidence.

Elsewhere (14) the writer has suggested that the associated items in the case of Pavlov's dogs conditioned to secrete saliva at the sound of a bell are not the bell and the food, but are

responses initiated by the bell (which responses act as stimuli) and the glandular activity. It is quite possible that the best formation of the law of association by contiguity will be in terms of coincidence of cue and response. We see no reason for amending the formulation of the law as previously suggested: Stimuli acting during a response tend on later occasions to elicit that response. To Razran's (21) suggestion that this is only a part of a more general law which would include the statement that on the first occasion stimuli acting when a dominant response is in process serve to energize that response at the time as well as to evoke it on later occasions the writer quite agrees.

Carr (5) has recently urged that more be made of the distinction between descriptive and explanatory laws. In his sense the law of contiguity stated in its 'qualitative' form is obviously an explanatory law, and stated in the form which Robinson proposes, it is a descriptive law. If we bear in mind that scientific explanations are made in terms of generalizations which are all essentially either descriptions of how events behave or they are no explanations at all, and if we therefore realize that Carr's distinction between 'explanatory' and 'descriptive' is really a distinction between broad and narrow generalizations, we may be tempted to solve the problem of the law of association by retaining both the more general form and specific quantitative forms for as many types of association as we have need to record. The hypothesis that association depends on coincidence of cue and act does not conflict with the statement that in any particular class of stimulus-response situations associative strength may be a particular function of associative interval.

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THE RÔLE OF THE PARASYMPATHETICS IN EMOTIONS

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The rôle of the sympathetic division of the autonomic nervous system and its chemical accessory, adrenalin, has been closely studied and described in relation to the production of the physiological symptoms of emotions, but to date the function of the parasympathetic division has suffered neglect. This neglect of the craniosacrals can hardly be explained by any intrinsic deficiency of this division in emotional activity, for vagotonic symptoms are as apparent as those of sympathetic origin. Kuntz holds that the parasympathetics are even more active in emotions than the sympathetic division (13, p. 439). Attention is here called to the activity of the parasympathetics because (1) it has been taken for granted that the sympathetic division is responsible for the major bodily changes in all emotions, regardless of kind; because (2) the parasympathetics are regarded as dominant only in quiescent, vegetative states of the organism; because (3) sympathetic activity is regarded as typically unpleasant and parasympathetic dominance as the condition of pleasant emotional tone; and because (4) the two divisions of the autonomic nervous system are presumed to be reciprocally innervated. These four hypotheses are presented in Professor Cannon's classic work on the *Bodily Changes in Pain, Hunger, Fear, and Rage*, and it is these hypotheses which, according to the writer's observation, contradict our daily experiences and which therefore we wish to contest. Vagotonic influence is observed to manifest itself, we think, (1) in the case of fear or terror, (2)in emotional depression or exhaustion, (3) in the characteristic expression of many individuals in almost any emotion, (4) in unpleasant emotionality, and (5) in mixed or double innervation with sympathetic activity, rather than in reciprocally pure forms.

Vagotonic Influence in Fear.—Professor Cannon writes throughout his book that the bodily changes are expressed without differentiation by the sympathetics. A comparison of the sympathetic syndrome, however, with a list of all the changes that take place in emotions will show that the sympathetic syndrome enumerates hardly more than half of the total visceral changes occurring. The other changes are produced by cranio-sacral stimulation. We propose the hypothesis that the differentiating physiological factor characterizing the bodily expression in fear is the large number of parasympathetic symptoms present. It is not proposed that fear is a pure parasympathetic syndrome, for this is obviously not true; it is suggested, however, that vagus influence is manifest in the physiological symptoms of fear, that it may even serve as a differentiating aspect of fear as compared with other emotions such as rage, that the degree of fear corresponds somewhat to the degree of parasympathetic influence, and that in the extreme changes occurring with terror, the parasympathetic influence is manifestly dominant over the sympathetic. We submit, hence, the following symptoms occurring in fear as evidence for the hypothesis, to wit:

(1) Cardiac inhibition is most obvious in fear states. Acceleration occurs in nearly all emotions, testimony to sympathetic influence, and occurs even in fear; but the more extreme the fear the more apparent is there a struggle between thoracic and vagus nerves for control, such that the heart now hammers away furiously and now halts. And in extreme fear or terror the inhibition is manifest in weak and tremulous pulsation and in sudden cessation. (2) A fall in blood pressure and syncope are produced by dilation of the large splanchnic vasomotor areas or by cardiac inhibition, either of which might exhaust the brain of its requisite supply of blood and thus produce loss of consciousness. Both splanchnic vasodilation and cardiac inhibition are produced by parasympathetic activity. Blood pressure low enough to produce fainting is incident only in those situations of such dire consequence that the individual feels overpowered and helpless; sinking of blood pressure and tremulous heart action may occur in less violent fear.

(3) A shift in the splanchno-peripheral balance of blood supply, from somatic to visceral concentration, with compensatory peripheral vasoconstriction, occurs frequently in fright. The responsibility for the consequent symptom of paleness or peripheral ischemia may be contested. It is usually taken for granted that, inasmuch as adrenalin or sympathetic stimulation produces vasomotor contraction, a decrease in blood supply to a given part is due to one of these agencies. But this is not the only factor to be considered in such a shift in blood distribution, for there must be a compensatory vasodilation effect, or a vagotonus, prevailing in another and equally large area, for the reason that the total volume remains constant and can not be compressed. The splanchno-peripheral balance in blood distribution follows the Dastre-Morat law, according to which, "the concentration of the blood in the splanchnic region by reason of paralysis of the splanchnic vasomotor mechanism, as has been assumed to occur in shock, is balanced by emptying of the peripheral blood vessels" (13, p. 469). The relative concentration of blood in the large vascular areas of the splanchnic distribution seems to be the determining factor in these general shifts while the somatic vessels act in compensation. If this be the case, then the nerve tonus prevailing in the splanchnic area must be the predominating vasomotor syndrome, and paleness as in fear should be due to the predominance of vagus stimulation.

(4) The concurrent blood pressure should be another criterion of which innervation is dominant and which is merely compensatory; hence the fall in blood pressure concomitant with paleness and fainting in fear indicates the shift in blood distribution to be primarily of parasympathetic origin. Further evidence for vasodilation in fear is supplied by Morse (17, p. 32). He points out that vasodilation in fear produces detumescence of erection in sexual excitement, diarrhea from intestinal vasodilation, diapedesis, and edema; multiple hemorrhages, blisters, urticaria, and purpura. Furthermore, the symptom of paleness is found regularly in vagotonic individuals; Jacobson's cases of strong spastic œsophagus, pylorus, and colon present this symptom characteristically with the emotional tendency to fear and anxiety (11, Ch. XVI).

(5) Spastic œsophagus is reported as a lump in the throat, as a feeling of the 'heart' sticking in the throat, as sensations of tightness in the throat and epigastrium, or as a sensation of choking. It appears suddenly in terror and gradually in grief and worry and other chronic fear states. Since the vagus and spinal accessory are responsible for the muscular innervation of the œsophagus, the symptom is obviously parasympathetic (**13**, **18**, **22**, *et al*). Jacobson finds that the fluoroscope shows immediate and objective evidence for this symptom in patients complaining of fear states (Ch. XVI). Spastic œsophagus is associated sometimes with another symptom of fear (**5**), aphonia, laryngospasm, hoarseness, or weakness of the voice. The 'dreadful scream of terror' which Darwin notes (**8**, p. 292) is due to this reflex spasm of the laryngeal muscles with the simultaneous contraction of the chest cavity. These symptoms are reported frequently in fear. They are mediated by the vagus, which supplies the larynx (**18**, p. 363).

(6) Professor Laird of Colgate University lately reports in a newspaper article the physiological evidence for the commonly reported 'sickening' or 'sinking' sensation attributed to the stomach; he finds an immediate gastric contraction occurring with an unconditioned stimulus for fear, namely, a sudden loud sound. (7) Intestinal hypermotility is likely to occur in fear and also in other extreme or prolonged emotional disturbances. There are many sensitive persons who pay for an emotional debauch with consequent diarrhea. This response in strong fear and shock may go so far as to produce involuntary expulsion of urine and feces. Alvarez notes some examples of the latter from ancient writings (**2**, Ch. I). Spastic constipation is frequently induced by emotional states, particularly by fear and anxiety. This symptom is not the result of general gastro-intestinal inhibition as would be expected from splanchnic dominance; it is on the contrary produced by the tonic spasm of circular muscles of the digestive tube, which prevents the normal passage of its contents. This symptom is hence listed under the parasympathetic syndrome (**18**, pp. 208, 257). Tonic spasticity is observed

under the fluoroscope in prolonged worry and fear states by Jacobson (11, Ch. XVI).

(8) Irritability of the urinary cyst, with frequent desire for micturition, is a symptom of common report in fear states. Sympathetic stimulation through the hypogastric plexus is manifested by relaxation of the digestive tube and of the urinary bladder with contraction of the sphincters of both, being thus the mechanism of retention, while parasympathetic stimulation produces the opposite responses, contraction of the digestive tube and of the detrussor muscles of the bladder, with relaxation of the sphincters of both, being thus the emptying mechanism (13, p. 283). The writer has observed a number of cases of chronic bladder irritability and reflex cystitis produced by fear and anxiety, while frequent urination is a very commonly observed symptom of fear.

(9) The general symptoms of asthenia and prostration are found most peculiarly in fear states. Muscular prostration is found in terror, and asthenia occurs in chronic worry, grief, and fear states. This general weakness and loss of skeletal tonus could hardly be taken as sympathicotonus, for this syndrome presents a picture of increased muscular tonicity, alertness, and excitability. Although no parasympathetic nerves seem to go to the skeletal muscles, on the other hand careful and repeated tests show that the sympathetic nerves which may be traced to striped muscles give no evidence of any influence upon the tonus of skeletal muscle (13, pp. 336–372). The variations in general skeletal tonicity must then be attributed to such general factors as blood pressure, cerebral excitability, hydrogen ion concentration, sugar concentration, temperature, metabolic rate, glandular secretion, etc., and low tonicity to parasympathetic depression of these functions; for it is the general function of the sympathetics to release and activate the bodily energies, while it is the function of the parasympathetics to depress these conditions of action.

Cannon's laboratory subjects, the cat and dog, are carnivorous animals and consequently are so constructed as to show the more robust, combative expressions of the emotions. They are accustomed to attack and not to defend, and their

emotional changes would appropriately be those of vegetative inhibition, somatic excitability, and discharge of metabolic energies, all of which constitute the sympathetic syndrome. The opposed parasympathetic symptoms of somatic weakness and visceral turmoil are useless nervous outlets of emotional stimulation and should occur rarely in these animals. We should expect to find them, ill-adapted, bankruptcy responses as they are, to occur only in situations for which there appeared to be no adequate possible response, only in such cases as extreme fear or terror, in which the rage reactions would be inadequate. To subject a cat to the menace of a barking dog is not to induce in the carnivorous and well-equipped cat a strong fear response. If she chooses to fight, the cat can put to flight a dog several times her weight. Her responses then should be called rage rather than fear. In the genuine fear state no such responses are called out; the cat whines, crouches, and struggles but does not even raise the fur on her back. The writer has subjected a number of these animals to strong stimuli that induce fear, such as close, darkened imprisonment with movement or electric shocks, and the only sympathicotonic symptom observed was pupillary dilation. Vagus activity was manifested by intestinal and urinary incontinence and consequent diarrhea. Vagotonia is still more easily produced in herbivorous animals and in the human being. These facts should indicate that, to obtain a true picture of parasympathetic or of extreme emotionality in animal experimentation some animal other than the predatory cat should be used, and more effective and various stimuli should be employed.

We are pleased to find others with a view similar to ours, a view which is contrary to accepted dicta concerning emotional expression. Bekhterev, for one, says, "a state of gladness arouses faster and stronger heart-beats—an effect which is produced directly by the increased sympathico-vagotonia, the sympathicus predominating. In fright the heart-beat is weaker and interrupted, the pulse is irregular and may stop altogether. In this case vagotonia, which occurs as a conditioned reflex, is highly predominant" (4, p. 281). Our

hypothesis is formulated by Ellice McDonald in a report on 'The Chemical Aspects of Life and Disease,' a report which represents the conclusions of a large body of collaborating psychiatrists, psychologists, medical doctors, and chemists. McDonald says, "In the study of emotion we have found that this also divides itself into two phases. Emotion of the type of despair or fear is characterized by activity of the smooth muscle of a vagus or active type, while anger or fighting fear is marked by sympathetic action shown in the immobility of the smooth muscle of the stomach. This last is the type of emotion which Cannon produced in cats when he showed there was an increase of adrenalin, a sympathetic stimulant in the blood" (**16**, p. 539).[1]

Vagotonic Influence in Emotional Depression and Exhaustion.—Whereas in strong fear stimulation we have general bodily excitement, in mild chronic fear we have a species of depression. In strong fear the somatic excitement is produced by sympathicotonus; visceral hypermotility appears at the same time through the cranio-sacral discharge which also, as we have already pointed out, occurs in intense emotion as fear. In mild chronic fear states, however, we have visceral turmoil and hypermotility without the somatic excitability of the sympathetics, *i.e.*, we have vagotonus almost solely in control. The resulting state is one which is commonly called depression. Externally there appears very little activity; there is, at least, little skeletal excitement, because this is made possible by a full release of bodily energies, which release does not in this instance occur because the sympathetics are not excited. In the absence of sympathicotonus, then, we have relatively little external activity; but if at the same time chronic fear emotion is discharging over the vagus, we may have what is usually recognized as depression but which is

[1] The group of collaborators which McDonald mentions includes: "Dr. S. D. Ludlum, Professor of Psychiatry; Dr. Elton Mayo, Rockefeller Fellow in Psychopathology; Dr. Josephine Gleason, Assistant Professor of Psychology in Vassar College; Dr. W. G. Karr, Assistant Professor of Biochemistry; Dr. Andrew Godfrey, Surgeon to the Chestnut Hill Hospital; Dr. W. Seifriz, Assistant Professor of Botany; Dr. Drowder of the Department of Clinical Pathology; Drs. Reinhold, Blodgett, and Saylor, chemists, as well as others" (McDonald, p. 457).

found internally to have a great amount of visceral hypertonicity and spasm and to present introspectively unpleasant internal feeling from the visceral tension.

The most important mechanism guaranteeing vagus predominance in depression and exhaustion states of emotion seems to be the acid-base balance that must be maintained in the blood and tissues of the body. This sanguinary neutrality, which is the *conditio sine qua non* of life, forces a recuperative parasympathetic dominance to restore the energies of the organism depleted by a too intense or too prolonged emotional or traumatic expenditure by the sympathetics. The acid bi-products of muscular activity such as lactic acid and an excessive amount of carbon dioxide which, in the form of carbonic acid, is the main factor in the acidosis of hypo-oxidative states, accumulates in the blood and causes the pH concentration of this amphoteric liquid to decrease. Since this potentially acid state is inimical to the life of the body cells, we have two mechanisms to compensate for acidosis. As Crile points out, hyperacidity depresses nerve functioning, such that further activity of the organism is discouraged (**7**, pp. 227–236). The nervous depression and the feelings of fatigue, inertia, and malaise serve as a protective mechanism. On the reconstructive side of the picture we have vagotonia, which reverts the organism to a recuperative state. Whereas alkalinity goes with sympathicotonia, acidity stimulates the cranio-sacrals (**13**, pp. 439–446; **15**, **16**). Hence we have vagus dominance regularly with emotional exhaustion states.

The parasympathetic symptoms of emotional depression in exhaustion states do not require enumeration. There are intestinal hypermotility, stasis, spastic œsophagus, constipation, diarrhea, pylorospasm, etc., and there may be reflex bladder irritability, paleness, low blood pressure, a weak and tremulous heart action, decrease in skeletal muscular tone, etc. We need only point out their vagotonic character. In the case of the anxiety neuroses and neurasthenia, our most common neurotic forms, these symptoms become chronic and indicate a vagotonic imbalance. In a paper and pencil test

devised by the writer and submitted to 800 students in psychology, the correlation between the emotional depression traits of neurasthenia and reflex vagotonic symptoms was .47.

For several cases of emotional depression with other neurasthenic symptoms, the writer experimentally prescribed small doses of ephedrine, a vegetable alkaloid which is a specific stimulant for the sympathetics, related in chemical constitution and in physiological action to adrenalin. A notable remission of emotional and asthenic symptoms was obtained. In one case, for example, with small pupils, restless, neurasthenic eye, pale and edematous skin, headache, indigestion, day-dreaming, emotional depression, etc., small doses of ephedrine relieved most of the symptoms and converted a boy on the verge of school failure into a good B plus student. The emotional depression was relieved *pari passu* with the physiological symptoms. The two are so closely associated that it is usually impossible to say which is cause and which effect.

Vagotonic Patterns in Individual Types of Emotion.—As Landis found a few types of emotional facial expression characteristic of the individual for his various emotions (14), so we find in the sphere of physiological changes a tendency for individuals to have a typical expression for various emotions. There is the lively 'expressive' extrovert who puts all his emotions into somatic and vocal action rather than visceral, who talks and gesticulates with great animation, and then cools off and forgets. This is the sympathicotonic individual, the freely mobile type. The vagotonic's emotionality is mainly marked by its strong visceral component, in particular by the alimentary hypermotility. Most commonly observed is the individual who suffers indigestion in some form from almost any emotional excitement. In one, it will be pylorospasm and retention of food in the stomach; in another it will be constipation; in another, diarrhea; in still another, spastic œsophagus, and so on. One case in mind is an individual who invariably is thoroughly purged after excitement of any kind, whether it be due to giving a public lecture or to a misfortune of a member of his family. Another individual suffers

severe bladder irritability and cystitis from strong emotion. Another person vomits in fear and anger alike. One case will have a sharp contraction of the stomach; still another will have constipation with mild emotion and diarrhea with intense ones. The vagotonic influence in these cases is so obvious, and the symptoms recur with such constancy that we are naturally led to postulate vagotonic patterns in individual types of emotion. We seem to acquire specific visceral habits just as we pick up characteristic verbal and manual habits.

Vagotonic Influence in Unpleasant Emotionality.—Professor Cannon maintains that the neural tonus conditioning pleasurable states accompanying eating, the quiet digestion of food, and the pleasurable sexual emotions is cranio-sacral, and that unpleasant affective states are induced by the overwhelming of vagus rule by reciprocating sympathicotonic discharge (**6**, pp. 334–339). Brunswick (**5**) has supported this view, and Allport attaches particular importance to it and formulates very definite laws to that effect (**1**, p. 90). Fear, however, is the most unpleasant emotion we have, and it is this emotion which manifests the greatest vagus activity. Joy and exultation on the other hand are attended by a full release of the sympathetics and rarely by any vagotonic symptom. The free, uninhibited expression of anger, also sympathicotonic, is nearly always pleasurable, according to the careful study of Richardson (**20**); unpleasantness occurs only in the phases of inhibition, phases likely to discharge themselves inwardly in vagus disturbances. The inhibited emotions are always unpleasant, and those freely expressed, with the exception of fear, are pleasant. Yet it is precisely the inhibited emotions and fear which, thwarted in free somatic expression, discharge internally with peculiar intensity and produce smooth muscle turmoil, that is, vagotonic discharge.

The individuals whose emotions characteristically affect the parasympathetics report these emotions to be unpleasant. According to Eppinger and Hess, the main exponents of autonomic imbalance, "The parasympathetic or vagotonic person has small pupils, bradycardia, cool, pale, clammy hands

and feet, sweats easily, is reserved and 'cold-blooded' and may suffer from asthma, constipation, gastric hyperacidity, etc. . . . The sympathicotonic individuals are tense, lively, excitable, with rapid pulse, large pupils, bright eyes, and a warm, pink, dry, skin" (**21**, p. 709).

The sexual reactions are not purely sacral, as Cannon himself admits (**6**, p. 338); they are both sacral and sympathetic, or balanced, involving both sacral and lumbar segments in the cord. The most pleasurable phase, orgasm, is mediated by sacral nerves and centers in the cord (**13**, p. 299; **6**, p. 338). As for the pleasurable states accompanying eating and digestion, we do not have an over-activity in either direction in these cases but a well-adjusted balance of control. Hence the only exceptions we find to the coupling of the vagus with unpleasantness and the sympathetics with pleasure are those cases in which we have a well-regulated neural equilibrium.

Vagotonic Influence Mixed with Sympathicotonic.—Professor Cannon holds that "there is evidence that arrangements exist in the central nervous system for reciprocal innervation of these antagonistic divisions, just as there is reciprocal innervation of antagonistic muscles" (**6**, p. 334). To support this hypothesis he goes on to say that normal digestive processes, which he presumes to exemplify vagotonia, are inhibited and superseded by sympathicotonus in emotions, and that sexual emotions may be suppressed by fear and rage. But normal digestion goes on under a well-regulated neural balance, not under the dominance of either; we have a medium activity of smooth muscle, and neither the total relaxation of sympathicotonia nor the hyperirritability of vagotonia. And the same applies to sexual reactions, which, as we have said, involve the double innervation of both divisions. How then could we say that they were mutually opposed or exclusive?

Criticisms of the concepts of vagotonia and sympathicotonia are widely recognized, and it is agreed that these states rarely appear in pure forms or act in mutual and reciprocal exclusion. Kuntz cites case after case of mixed syndromes and concludes that the original theory of reciprocity and

mutual exclusion between the two divisions can no longer be tenable (**13**, p. 439). Hence we protest against the theory that the sympathetics act in clear and pure form in emotions, and a theory that the parasympathetics appear in such clear or pure forms would be met with equally strong protest. Even Professor Cannon admits to certain exceptions to his hypothesis (**6**, p. 338). If we look further, however, we find the exceptions to multiply, and we find mixed states to be the rule rather than the exception. We only propose that, in a given complex state of emotion, the elements of fear and depression may be indicated by the relative amount of vagus influence manifest.

Conclusion.—(1) We find parasympathetic symptoms in fear and hold that the emotion may be gauged viscerally by the vagotonic symptoms occurring. (2) Vagotonia is also present in emotional depression and exhaustion states. (3) There are many individuals who habitually express almost any kind of emotion by a characteristic pattern of vagotonic disturbance. (4) Vagus dominance is extremely unpleasant in affective tone, while sympathetic dominance may be pleasurable. (5) Finally, we find no evidence for the reciprocal innervation of the two divisions of the autonomic nervous system; we find, instead, a double excitement and mixture of symptoms to be the rule.

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USE AND LIMITATIONS OF EYE-MOVEMENT MEASURES OF READING

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Since the early work of Erdmann and Dodge, eye-movement records have been employed more and more as measures of reading performance, especially during the last 15 years. There are now available sufficient data for a fairly adequate evaluation of this technique. The purpose of this paper is to analyze the use and point out some of the limitations of eye-movement measures of reading.

There are four general methods for obtaining records of eye-movements and pauses during reading: (1) Direct or indirect attachment of a mechanical recording apparatus to the eyeball; (2) photographing (*a*) eye with point of reference attached to eyeball, or (*b*) beam of light reflected from mirror held gently against closed lid of one eye; (3) counting eye movements from observation of eye with or without auxiliary aids (*i.e.*, mirror, telescope); (4) photographing the image of a light reflected from the surface of the cornea.

It is not necessary to list in detail and to criticize the various methods of recording eye movements since that has been done elsewhere (**5**).[1] Because of technical difficulties, inability to use a method with more than a few subjects, excessive labor in reading the records, and possibility of producing unrepresentative performance, most of the techniques developed for recording eye movements have had slight application in research or clinical practice. Some variations of the third and fourth methods listed above, however, are in rather general use.

[1] In most instances specific citation of reference will not be made in this paper. For a complete list of studies on eye movements and descriptions of apparatus see: M. A. Tinker, Legibility and eye movement in reading, *Psychol. Bull.*, 1927, **24**, 621–639; Physiological psychology of reading, *Psychol. Bull.*, 1931, **28**, 81–98; Experimental study of reading, *Psychol. Bull.* (in press).

In the corneal reflection technique, which is easily employed with large numbers of subjects, is produced a true representation of number, duration, and sequence of pauses in reading. Many experimenters, as an aid to analysis of reading performance, have plotted from these photographic records the locations of fixations on the text read. There is a tendency among many readers to interpret these locations of fixations too literally. There is no such thing as a fixation point in reading, but rather a fixation field. Furthermore, as Dodge and others have pointed out, there is some error involved in determining from the photographic records the exact location of the eye in relation to the reading copy. Part of this error is inherent in the photographic technique and part in inaccuracies of matching record with text read. So the reliability with which the pauses are located is somewhat limited. In analyses of this kind, therefore, the indicated locations of fixations must be considered to represent merely the *approximate centers of fixation fields*. This kind of analysis is useful but should not be misinterpreted.

Because photographing the corneal reflection requires an elaborate, expensive, and non-portable apparatus, the direct method of observing and recording eye movements has been used considerably. The method is valuable mainly in two kinds of studies: (1) for clinical observation of eye movements (fixations and regressions) in which very good and very poor readers are easily differentiated from the average; (2) to study trends revealed by group comparisons and correlations. The experimenter, of course, should be well trained in observing and recording these movements. The validity of records obtained by direct observation has been investigated by Tinker (6) and Robinson and Murphy (4). Their data demonstrate convincingly that, although trends are quite adequately revealed, about one-fifth to one-half of the movements are missed by the experimenter doing the counting. This technique should *never* be used, therefore, to measure the absolute number of fixations and regressions made by readers.

The most important use of eye-movement measures has

been to discover the fundamental nature of oculomotor habits in various reading situations such as: development with age, in proof reading, variation with reading context and attitude, and the like. An important series of studies have begun to appear on eye movements in special reading situations such as reading: numerals, formulæ, music, and in spelling and typing. This type of study promises to be even more fruitful in the immediate future. One of the most interesting and instructive phases of these investigations is the analysis of patterns of movements which are characteristic of the particular reading situation. In these studies, speed of reading which is represented by perception time and to a lesser degree by fixation frequency, is probably of minor significance in comparison with patterns and sequences of movements.

The character of eye movements indicate to some degree the nature of perceptual processes during reading. Eye-movement habits are very flexible and appear to adjust themselves readily to any change in the perceptual processes involved. Clear and rapid comprehension of textual material in reading is accompanied by relatively few and short fixation pauses spaced in rhythmical sequences. Difficulty in comprehension, confusion of mental processes during reading, slowing up of apprehension, and the like, are immediately reflected in variation of number and duration of fixations, and complexity of eye-movement patterns. Analysis of perceptual processes in reading by means of eye-movement records will continue to hold an important place in studies of the reading process.

Reading deficiency, immature reading habits, and improvements of reading efficiency are readily detected by measuring the reader's eye movements. Considerable use has been made of this technique in diagnosing reading disability and checking improvement. It is questionable, however, whether eye-movement records contribute materially in diagnosing reading disabilities in the clinical situation. We know that the eye movements of the non-reader or the retarded reader are characterized by aimless wandering, by many fixations and regressions, and the like. Because eye-movement habits

readily adapt themselves to changes in reading ability, we can predict that improvement in reading will be accompanied by more orderly oculomotor performance. It is probable that, in practically all cases, faulty eye-movements are merely symptoms rather than causes of poor reading. It appears, therefore, that measurement of eye-movements may be dispensed with in the reading clinic without any appreciable loss.

There have been several attempts to relate eye-movement scores to various kinds of achievement such as scores on standardized reading tests, intelligence test scores, and school grades. Equivalent results may be obtained without measuring (photographing) eye-movements which requires elaborate apparatus and is expensive. Perception time (sum of pause durations) and fixation frequency are essentially measures of speed of reading (7). There are, of course, several standardized performance tests for measuring speed of reading. It is more convenient in every way, therefore, to employ standardized tests rather than the eye-movement technique in investigating the relation of speed of reading to other achievement scores. Even in reading more difficult material for comprehension, speed is more easily measured with a stop-watch than by photographing the eye movements.

In eye movement analyses of reading there are four measures recognized: fixation frequency, pause duration, perception time (sum of pause durations), and regression frequency. As noted above, perception time and fixation frequency are essentially measures of reading speed. Hence they are satisfactory measures of reading performance. Pause duration, although it reveals certain interesting and important characteristics of oculomotor patterns, is of minor value as a quantitative measure of reading. This is probably because pause duration is not a satisfactory measure of reading speed, except in combination with fixation frequency which produces perception time. Regression frequency is only a fair measure of reading performance in the straightforward reading of prose. In special reading situations regression frequency has little or no relation to speed of reading and consequently has little value as a quantitative measure of reading. How-

ever, as with pause duration, regressions reveal important characteristics in eye-movement patterns.

Until quite recently the question of reliability of eye-movement measures has not been raised. In all the earlier studies it was apparently assumed that photographic and other measures of eye-movements in reading were sufficiently reliable to justify rather definite conclusions from the data collected. There is little evidence to justify such an assumption. It seems reasonable to suggest that a measure of reliability be obtained whenever possible in each reading situation investigated. Recently published reliability coefficients for measures of eye-movements are high. Correlating first half against second half of the test, Litterer (**3**) obtained coefficients (raised by S-B formula) of .91 and .92 for perception time and fixation frequency on easy prose, and .91 and .78 for the same measures on scientific prose. Reliability coefficients of comparable magnitude are cited by Eurich (**1**) and Frandsen (**2**). No test-retest reliabilities have been published.[2]

There is an important precaution to observe in computing and interpreting reliabilities of eye-movement measures. It has been well established that eye-movements are very rhythmic and that both frequency and duration of pauses are consistent when reading becomes automatic and is done without comprehension. Such reading would yield high but spurious reliability coefficients. This emphasizes the need of controlling the reading attitude by instructions and by a careful check on comprehension.

Apparently it has also been assumed, almost without question, that eye-movement records are valid measures of reading ability. There is available some indirect evidence which indicates that the validity of these measures is quite high. Tinker (**8**) has pointed out that speed and comprehension in reading appear to be intimately related when adequate methods of measurement are employed. Therefore, since eye-movement records measure mainly speed of reading, it is reasonable to infer that they have a good degree of validity either as measures of speed or comprehension in reading.

[2] The writer will soon publish an extensive study on reliability and validity of eye-movement measures.

There are, however, more direct data on the validity of eye-movement measures of reading ability. Validity coefficients may be computed in two ways. In the first, scores on standardized tests of speed and comprehension in reading may be employed as criteria. When this is done certain precautions concerning interpretations should be emphasized. As Tinker has indicated, there are many reading skills "which are somewhat independent, rather than either a general silent reading ability, a general comprehension ability, or a general speed of reading ability" (**8**). When the scores on one reading test are correlated with scores on another composed of material which is not strictly comparable to that in the first, irrespective of whether speed is correlated with speed, comprehension with comprehension, or speed with comprehension, the highest coefficient to be expected is approximately .60, but it may be considerably smaller. Coefficients between .25 and .45 are quite representative of these correlations. Therefore, when eye-movement records are correlated with scores on standardized tests (of different textual material), a coefficient of .25 to .40 should indicate fairly high validity, and a coefficient of .45 to .55, excellent validity of the eye-movement records as measures of reading ability. Litterer's (**3**) validity coefficients, computed in this manner, range from .27 to .51 with a median of .42 (twelve coefficients). These data justify the inference that eye-movement records (perception time and fixation frequency) have fairly high validity. Eurich's (**1**) validity coefficients are low (.02 to .25) and are difficult to interpret.

The second type of criteria that may be used in computing validity of eye-movement measures of reading ability consists of speed and comprehension scores on material strictly comparable (an equivalent form) to that read before the camera. This second method of computing validity seems to the writer to be more adequate than the first mentioned because the results are bound to be more unequivocal. No validity coefficients derived by this technique have been published but incomplete data collected at the University of Minnesota suggest that the validity is probably very high.

Because the measurement of eye-movements is a costly and laborious procedure most of the quantitative studies involving group comparisons have employed relatively few subjects. In most of the groups studied the number of subjects have ranged from four to about twenty. Since there are marked individual differences in all kinds of reading, the unstable group averages derived from such small numbers of cases afford bases for only tentative conclusions. Quantitative studies with larger populations will help to establish on a firmer foundation or to rectify the trends discovered in the earlier studies, and will furnish an important method of attack in the investigation of new problems.

A survey of the published investigations yields convincing evidence that recording eye-movements has become a fairly common and a very useful method of studying reading performance. The variety of problems investigated demonstrates that the technique is quite flexible. We have seen, however, that there are pitfalls in the method which, unless avoided, are apt to lead to invalid results and unconvincing conclusions. Therefore the investigator should give particular attention to an adequate control of experimental conditions, and use only valid methods of analysis in examining his data.

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DISCUSSION

CONATUS IN SPINOZA'S *ETHICS*[1]

The problem of determining the precursors of any intellectual movement is beset with two difficulties: the investigator may either, with American laziness, attribute novelty to every cult and vagary, or he may, with German diligence, trace back everything to the *Republic*, the *Book of the Dead*, and the *Code* of Hammurabi. This note should avoid both absurdities by presenting its portrait of Spinoza the Psychoanalyst as an antiquarian coincidence in the history of thought.

The *conatus*, translatable as Will, Energy, Desire, Center of Force, or better yet as *conatus*, enters Spinoza's *Ethics* in Book iii, long after the mind-body problem has been solved and metaphysics reduced to certainty. As Spinoza justly remarks, all the tracks lead into the cave of his rationalistic Absolute, and there is no path back to the individual. Some additional principle is needed to keep the individual from disintegrating, some instinct of self-preservation which, on the level of confused ideas, will keep body and soul together and alive, and, on a clearer level of thought, will prevent body and mind from losing themselves in the Divine Attributes of which they are rightfully a part. And that principle is the *conatus*. "The *conatus*, whereby a thing endeavors to persist in its own being, is nothing else but the actual essence of the thing in question."[2]

This raising to the position of essence of a non-rational part of the self which in Descartes had been an accident of the self, and a source only of error and illusion, was typical of Spinoza. For (*a*) his rationalism was of a cosmic nature; he had little need or desire to make rational the mere individual existence. And (*b*) neither theological nor political considerations prompted him to pay compliments to the souls of his contemporaries.

"This *conatus*, when referred solely to the mind, is called Will, when referred to mind and body together is called Appetite. . . .

[1] "It is amusing to notice that Spinoza's *Ethics* contains precisely the theory which Mr. Bertrand Russell puts forward in his *Analysis of Mind* as a wonderful new discovery which we owe to Psycho-analysis," C. D. Broad, Five types of ethical theory, p. 24.

[2] Bk. iii, Prop. VII. Elwes translation throughout.

And between Desire and Appetite there is no difference, except that the name Desire is applied to Appetite when there is consciousness thereof. . . . In no case do we wish for, strive for, long for, or desire anything because we think it good, but rather we deem it good because we wish for, long for, or desire it."[3] Thus we find the *conatus* is the source of our appetites and desires, and is a fundamentally *unconscious* entity, to which consciousness is only on occasion added. And it is not a product of the pain-pleasure principle, but rather pleasure and pain are functions of its strivings. (Consider psycho-analytic explanations of perversions.)

And next we find this: "When the mind conceives things which diminish or hinder the body's or mind's activity, it endeavors, so far as possible, to exclude it from consciousness."[4] The internal censor, in somewhat crude form, makes its first bow. "Often we love or hate a thing without any cause of our emotion being known to us, merely, as the phrase runs, out of sympathy or antipathy."[5] Here follows an explanation of subconscious emotional sets in ms of association mechanisms. But later,[6] we find that similarity will induce an emotional transfer also, even "though the point of resemblance be extraneous to the emotion, and unconscious." The whole psycho-analytic doctrine of transference, symbolism, and distortion can without much difficulty be uncovered in that proposition. The subconscious *conatus* seizes upon some point of resemblance between two entities, and transfers its emotional set from one to the other, without awareness even of the connection.

There follows, in Book iii, much irrelevant material, but the book closes with a neat summary. "Desire is the actual essence of man. . . . Desire is Appetite, plus the consciousness thereof. . . . But strictly speaking I note no difference between Appetite and Desire, for Appetite is the same Appetite whether conscious or no. By the term Desire I mean, further, all men's endeavors, appetites, volitions, and impulses."[7]

Beset with difficulties is any definition of normality in the study of abnormal psychology. One definition often advanced in psycho-analysis is this: "Normality is the ability to perform satisfactorily the functions of one's species and society." That is

[3] Bk. iii, Prop. IX, *scholium*.
[4] Bk. iii, Prop. XIII.
[5] Bk. iii, XV, *scholium*.
[6] Bk. iii, Prop. XVI.
[7] Definitions of emotions, I and *scholium*.

the precise sense of Spinoza's definition of the good, the perfect, the 'normal' man.[8] And the whole of Book iv, significantly called *Of Human Bondage*, deals with man's bondage to his emotions, to the darker and less conscious volitional aspect of his nature.

In Book v, *Of Human Freedom*, the method of subduing this *conatus*, of purifying the libido, is discussed, and that method is Freudian through and through. "An emotion, which is a passion, is a confused idea. . . . If, therefore, we form a clear and distinct idea of the emotion, that idea will be distinguished from the emotion, insofar as it is referred to the mind only; therefore the emotion will cease to be a passion." [9] "Corollary: An emotion therefore becomes more under our control, and the mind is less passive in respect to it, in proportion as it is more known to us." [10] And so Spinoza too thought that the dragging to light of the contents of our unconscious would free us from our irrationalities. His explanation, a bit confused, seems to be that once the idea is dissociated from the passion in the *conatus*, and viewed impartially, the connection is forever broken.

One more parallel between Spinoza and Freud. What is the final solution of the problem of this perverse, confused, unconscious *conatus* or libido? The good life is the *amor intellectualis dei*, the intellectual love of God, which bears resemblance to the heights of emotional ecstasy as well as to the heights of intellectual contemplation. And the emotional and ecstatic part of that highest form of knowledge and of being comes through divorcing the *conatus* from its limitations and self-preoccupation, and turning it, *sublimating* it, towards higher ends.

In short, in the concepts of the *libido*, of the *unconscious*, of the *functional norm*, of *association and transfer*, of *cure through self-knowledge*, and of *sublimation*, Spinoza anticipated Freud, and can be called an early psycho-analyst.

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[8] Bk. iv, *preface*.
[9] Bk. v, Prop. III.
[10] *Loc. cit., corollary*.

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